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A FIELD DEMONSTRATION OF THE EFFECTS OF NONAUTOMATED RESPONSIVE ENVIRONMENTS ON THE INTELLECTUAL AND SOCIAL COMPETENCE OF EDUCABLE MENTALLY RETARDED CHILDREN.

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A PRESCHOOL PROGRAM WITH CHILDREN OF LOW-SOCIOECONOMIC STATUS WAS CONDUCTED TO ENHANCE THEIR EDUCABILITY. THE SAMPLE (N 74) WAS DIVIDED BY RANDOM ASSIGNMENT INTO ONE CONTROL AND TWO EXPERIMENTAL GROUPS. PROVISIONS WERE MADE FOR AN OPTIMAL NURSERY SCHOOL ENVIRONMENT BY (1) HELPING CHILDREN TO FUNCTION SOCIALLY IN A GROUP, (2) PARTICIPATING IN EXPERIENCES DESIGNED TO AROUSE CURIOSITY, AND (3) TRAINING FOR ACQUISITION OF ACADEMIC SKILLS. DURING THE 3-YEAR PROGRAM, A NUMBER OF MEASURING INSTRUMENTS WERE EMPLOYED--(1) COGNITIVE (APTITUDE, ACHIEVEMENT, LANGUAGE), (2) NONCOGNITIVE (PERSONALITY, SOCIAL), AND (3) ENVIRONMENTAL (HOME, FAMILY). THE GROUPS WERE NO DIFFERENT AT THE CONCLUSION OF THE STUDY THAN THEY WERE AT THE BEGINNING. ALSO, NO EVIDENCE WAS OBTAINED TO SUPPORT THE THEORY THAT INTELLIGENCE SCORES CAN BE IMPROVED. IT WAS SUGGESTED THAT ADDITIONAL EFFORT SHOULD BE GIVEN TO THE NATURE-NURTURE INTERACTION, THE TIME TO BEGIN INTERVENTIONS, AND POSSIBLE INTERVENTION MODELS. (JK)

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A FIELD DEMONSTRATION OF THE EFFECTS OF NONAUTOMATED RESPONSIVE ENVIRONMENTS ON THE INTELLECTUAL AND SOCIAL COMPETENCE OF EDUCABLE MENTALLY RETARDED CHILDREN

Cooperative Research Project No. D-014

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Boston University
Boston, Massachusetts

1965

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Preface

We are indebted to Alfred Binet as the progenitor of much of the inspiration for the research reported in this volume. His book, Les Idees Modernes Sur Les Enfants (1909) is testimony to the ancient adage that nothing under the sun is new. Binet, creator of the prototype instrument to "expose" mental defect, took greater pride in his pedagogy for what he called "educating intelligence." He, who did more than other scientists of his day, to quantify the price--the consequence--of mental retardation knew that his achievement was trivial in contrast with unraveling the puzzle of mental retardation, i.e., in discovering those interventions that might deal most effectively with the problem of intellectual limitation, that might educate intelligence or that might prevent intellectual disorders. On the one hand he knew that if little was done for the intellectually limited, if there was not a strong active educational intervention, these children would waste time in school, would become discouraged, and eventually would lose all taste for any type of work--a calamity not easily correctable after formal school termination. On the other hand, he was aware of, and regretted, the widespread prejudice of teachers against the notion that intelligence was educable. In spite of impressive evidence, inferred from a host of psychological experiments, teachers were indifferent to the hypothesis that, with training and practice, one could develop faculties of memory, attention and judgment and, thus, become more intelligent than heretofore. Stated another way, Binet deplored the general dismissal of the hypothesis that an important part of the variability of intelligence is attributable to training and practice.

Binet's pedagogy, "Mental Orthopedics," sought to increase the intelligence of the mentally retarded child--until "...one has attained one's limit, for it cannot be denied that there is one ..."--by straightening, cultivating, and strenghtening the attention, memory, perception, judgment, and will of this child. His curriculum design gave answers to three questions: Who is the teacher? What does she teach? What is her method? Binet viewed the teacher as an observer, one who knows each child in the class, one who is able to adapt instruction to meet the individual needs of each child. To avoid discouragement, confusion, and bad work habits of pupils, she places instruction on the pupil's level of ability. She begins with the known and proceeds to the unknown. She does not take for granted that all children come to school with a background of experiences that are conducive to successful learning in the school setting. Her method is not to teach children *the* idea which seems most *useful* to them. Her primary duty is to provide an environment where children "learn to learn." Hers is a "war on verbalism" and passivity. Her methods demand that students be active, be discoverers. Truly, Binet's pedagogy centered upon the provision of what modern educators term the "discovery method," i.e., the reliance on the child's natural curiosity and ability to explore his environment, come to grips with the meaning of it, and develop his faculties for learning through this experience.

Why did Alfred Binet concentrate his energies and genius on the study of mentally retarded children? First, he expressed a great and natural pity for this unfortunate group of youngsters. Secondly, he had a strong desire to build what he called "a social defense," a plan to reduce the number of those who otherwise would be expected to become burdens of society. However, his chief interest in this field concerned itself with the firm belief that the study of retarded children serves all children; the pedagogy that can educate their intelligence may educate the intelligence of typical and bright children.

In principle, the objectives and justifications of our research are identical to Binet's. What Binet felt as "pity" we describe as compassion for an overlapping group of youngsters who are continuously confronted with impossible tasks both in the school setting and in their attempts to create independent and contributing lives for themselves--the mentally retarded and the culturally disadvantaged. Secondly, in this age of increasing automation and industrial technology, we are grievously disturbed by the political, social, and moral consequences of a jobless, disfranchised, and helpless segment of our society. Lastly, we are in full agreement with Binet that discoveries made concerning the educability of intelligence are applicable to all children, not only those with the most overt need, the mentally retarded and disadvantaged.

To summarize, it was the vision of Alfred Binet to design an environment that would be powerful enough to intrude upon the lives of retarded children in a way that would reduce their massive inability to comprehend and profit from the school setting. With two major exceptions--one in the realm of research strategy and tactics, the other theoretical--this too is our vision. Binet centered his pedagogy on the treatment of the consequence of mental retardation. Our attention is to the treatment that may serve to prevent the condition from arising. Secondly, we cannot assume a fixed intellectual ceiling. It is not that we wish to argue against the notion of some finite point, beyond which human beings cannot attain. However, we have observed that, when dealing with the concept of "capacity," the history of behavioral science is replete with a characteristic underestimation of human achievement. Unfortunately, these underestimates are often designated as "over-achievements," an obvious non sequitur. At the present time, with our limited experience in dealing with the ability domains of man, we believe it far more prudent to

leave open the question of his capacity.

It was the original purpose of this research to locate a group of preschool children drawn from families designated as "cultural familial mentally retarded" and to provide them with a variety of experiences calculated to engender and reinforce attitudes, motivations, and cognitive skills considered prerequisites for normal intellectual and academic growth. We expected that, in comparison with an appropriate control group, experimental children would display significantly lower intellectual and academic deficiencies so frequently encountered among children reared in such families. Our basic premise was that intelligence is educable and that if we could provide a sufficiently powerful intervention for children "destined" to become mentally retarded, this retardation could be prevented or, at least, mitigated. We learned during the course of this project that, as our research proceeded, its direction and focus deviated more and more from our original purpose. As descriptions of our strategies, tactics, and experiences unfold in this monograph, we expect it will become clear to the reader that the title of this report reflects our original conceptualizations and intentions rather than how we actually proceeded and what we learned. Unfortunately, in order to preclude retrieval problems for individuals trying to locate reports of federally sponsored research, titles of such final reports must be identical with ones appearing on original contracts. The following should communicate our dilemma insofar as a discrepancy exists between the title of this report and its contents.

If, after the first year of this research, we were asked to submit a monograph on our findings we probably would have titled it, Educating Intelligence: A Study in the Prevention of Mental Retardation. Had we the freedom to title this monograph now in whatever way we wished, there is no

doubt that we would select a title which has reference to the Determinants of School Behavior of Disadvantaged Children.

The research we report in this monograph--made possible through a grant from the United States Department of Health, Education and Welfare, Office of Education, Cooperative Research Branch and considerable consultive and personnel support from the Division of Mental Hygiene of the Massachusetts Department of Mental Health--incurs for us numerous debts of gratitude that we will find difficult to repay. Dr. William Carriker, formerly of the United States Department of Health, Education and Welfare and now Professor of Special Education at The Pennsylvania State University, and Drs. B. R. Hutcheson and Lewis B. Klebanoff of the Massachusetts Department of Mental Health were staunch and sympathetic supporters during the years they served as liaisons between their agencies and our project staff.

To Dr. Malcolm Farrell, Superintendent, Mr. Lawrence Gomes, former Principal, and the staff of the Walter E. Fernald State School, Waverly, Massachusetts, we are indebted for the valuable space given us to establish a laboratory at Fernald and for their cooperation and goodwill. We are grateful for the cooperation given us by the Newton, Massachusetts Public Schools, its staff and, especially, Miss Margaret Otto, Director of Special Education. We are equally appreciative of the cordial reception we received from the Waltham, Massachusetts Public Schools, and in particular, its Director of Special Education, Miss Eleanor Malloy. Certainly, without the support and skill of Miss Elsa Baldwin, Director, the staff, and volunteers of the Cambridge Neighborhood House, there would have been untold difficulties in selecting an adequate subject population.

We have great appreciation for Dr. Omar K. Moore now at The University of Pittsburgh, and his staff at the Responsive Environments Laboratory at

Hamden Hall Country Day School. The generosity in permitting us to observe their project, in agreeing to participate in the training of our booth supervisor and in sharing with us their theories and techniques place us in their debt.

For all of the booth teachers--too numerous to mention--who served our project so conscientiously and skillfully we have the deepest respect and admiration. For her exceptional service to the project, for her deep understanding of human nature and the frailties of human beings, for her skill in dealing with children and staff, we wish to single out Miss Ruth Wong, head booth teacher, and express our deepest gratitude.

To our head classroom teachers who gave unselfishly of their wisdom, skill, and compassion--Mr. Samuel Wakshull, Mrs. Jean Friswell, Mrs. Ruth Allen, Mrs. Beverly Bates, and Mr. Harold Woodward--we offer our praise for efforts above and beyond those normally expected of professional workers.

To those who directed our psychological testing, case finding, and family evaluations--Mr. John Ogonik, Dr. Richard Brodie, Miss Sandra Haughton, Dr. Thomas Mahan, Mr. Robert Wise, Miss Helen Garretson--may we express our sincere appreciation for competent clinical endeavors and supervision of staff. We appreciate the willingness of Dr. Newell Squires in handling both the medical evaluations of our subjects and the numerous minor illnesses and injuries that occurred during the project years. We would be most ungrateful if we did not offer appreciation to Mrs. Dorothy Tucker for the support and supervision she gave our classroom teachers and to Mrs. Ethel Dwyer for the development of an excellent music program for the project classes.

To Mr. George Van Son, Miss Lucy Juralewicz, Mrs. Ellen Letterman, Miss Betty O'Keefe, and Mrs. Nancy Perkinson, we are grateful for patience with us, Hamden Hall Country Day School. The generosity in permitting us to observe their project, in agreeing to participate in the training of our booth supervisor and in sharing with us their theories and techniques place us in their debt.

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To Mr. George Van Son, Miss Lucy Juralewicz, Mrs. Ellen Letterman, Miss Betty O'Keefe, and Mrs. Nancy Perkinson, we are grateful for patience with us, for efficient handling of our office problems, for typing of our manuscripts, and for level-headed competence.

Dr. George Brabner, currently Associate Professor of Special Education at Yeshiva University, served this project for two years as research associate. Through his efforts, we were able to keep rather careful records of the activities of teachers, their curriculum plans, and classroom programs. We are grateful for the attention he gave to the many and varied mundane day-to-day problems that projects such as ours always encounter. Further, he was responsible for a significant share of the theoretical formulations and implementations relating to the development of our curriculum.

Dr. Seymour Sarason, Professor of Psychology at Yale University, was much more than a psychological consultant to this study. His earlier conceptualizations, summarized in Psychological Problems in Mental Deficiency (1959), provided us with a theoretical reference and ideational stimulation to design and develop this research. Further, he devoted five years as our primary source of consultation, feedback, and encouragement.

To our former dean, Dr. Max Goodson, currently Professor of Educational Policy Studies at the University of Wisconsin, and our current dean, Dr. Jack Childress, the staff, and Special Education faculty at the Boston University School of Education, we apologize for the inconveniences we may have caused during the course of this research. In spite of our disconcerting maneuvers, their cooperation and goodwill have been unflagging.

Our greatest indebtedness is to the families who permitted us to intrude upon their lives and who entrusted us with their children, without whom this project would have been neither possible nor necessary. We dedicate this book, not only to what their children are but to what they may one day become.

Boston
December, 1965

B.B.
F.G.

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Chapter I

Introduction

The present study, although limited in scope, is concerned with aspects of the very broad, complex, and significant problem of the relationship between social class background, on the one hand, and intellectual and academic growth, on the other hand. More specifically, we are concerned with some of the ways in which intervention into the preschool and early school lives of lower-class children reduces the likelihood that such children will develop intellectual and academic deficits--qua mental retardation--so frequently found in children from such backgrounds. Before proceeding to a detailed description of our study and the body of research to which it is relevant, it would seem appropriate to discuss briefly the fact that applied social science has only begun to take seriously the concept of social class status or background.

One of the most distinctive and important problems which has come to concern American social and behavioral scientists involves the nature and correlates of social class status. Much of the pioneer work in this problem area comes from fields (e.g., sociology, anthropology, political science) little concerned with the practical implications of research findings, which is not to say that workers in these fields were unaware that a fundamental concept such as social class had obvious significances for political action, social planning, and social organization. More often than not, it was not until a particular problem erupted in varying degrees into national consciousness that it was viewed and studied in terms of social class variables with the aim of developing a more effective way of coping with the problem.

Mental retardation is a clear example of an important problem area which only in recent years has been systematically discussed in terms of the social class variable. For decades, mental retardation was of interest primarily to

certain medical, psychological, and educational specialists as well as to state administrators who tended to view institutional custody as society's major way of coping with the diverse aspects of the problem.

Only since World War II has mental retardation come to be recognized as a national problem and one which has to be viewed and studied within the context of our social traditions, practices, and organization. This is but another way of saying that as our society became aware of the enormity of the problem--in terms of the number of individuals involved and the inadequacies of our knowledge--it was implicitly assumed that mental retardation would be freshly viewed and studied by the social and behavioral sciences and that knowledge would be forthcoming which would be the basis of more effective social action. (A similar assumption was made about the biological aspects of the problem and in light of the research done in the past 15 years there is little doubt that this assumption was a valid one.)

It would be a gross distortion of the history of the field to imply that interest in the relationship between mental retardation and social class is of recent origin. Such an interest was early guaranteed, so to speak, by the inescapable fact that the largest subgroup in the mentally retarded population was found in a certain part of our society. We refer here, of course, to what over the years has variously been called the garden-variety, or subcultural, or cultural familial, or familial defective individual. At least five characteristics of these individuals have long been described: (a) by traditional methods of evaluation their intelligence was subnormal, (b) the intellectual level and social adequacy of the parents appeared also to be subnormal, (c) there was no discernible central nervous system pathology, (d) they were born into, and reared in, a cultural milieu which was "inferior" to

other strata of our society, and (e) they represented a disproportionately large part of the case load of many social agencies. Unfortunately, the awareness of a relationship between this type of mental retardation and social-cultural variables was neither related to theoretical and research developments on the concept of social class nor resulted in research on the problem by those in the field of mental retardation. This is hardly surprising when one considers that it is only in recent years that the field of mental retardation has shown signs of becoming part of the main stream of thinking and research in the behavioral sciences. For example, it was not until 1956 (Masland, Sarason, & Gladwin) that we encountered the first systematic effort to view mental retardation from the standpoint of social-cultural variables and research. That this was the work of an anthropologist (Gladwin) is not without importance in light of the earlier isolation of the field of mental retardation from the behavioral sciences.

The early recognition of the relationship between a particular type of mental retardation and social class factors (implied in the label "subcultural") quickly became caught up in the nature-nurture controversy. This controversy will be reviewed in the next chapter and does not need discussion here. What is important about this controversy is that it did not give rise to systematic attempts to study the problem by planned interventions and changes in this particular social milieu. Interventions in the forms of sterilization and separation of child from family by institutionalization were not done for the purposes of research. In addition, the consequences of such interventions, however justified they may be in the individual case, raised as many (if not more) problems than they resolved. In any event we are not aware that such interventions have thrown light either on the nature-nurture problem or on the role of social class factors on the type of mental retardation of which we have been speaking.

The situation today is quite different. Whereas formerly there was awareness is reflected in a variety of research efforts to study lower-class cultures in order to clarify the relationship between social class variables and intellectual and academic performance. Several factors account for this encouraging change in research emphasis. First, our society, having come face-to-face with the enormity of the problem of mental retardation, has begun to make funds available for research at a level undreamed of in the past. Second, the recognition that institutionalization was a grossly inadequate means of handling the problem, particularly in regard to the lower-class mentally retarded child, emphasized the need for studying ways of maintaining these individuals in the community. Third, if community schools were to be effective vehicles for the socialization of the lower-class child--i.e., to maximize his potential for intellectual and academic growth--the pedagogical approach had to take into account the culture from which the child came. Fourth, there was increased recognition of the possibility that, if social-cultural variables were important in the intellectual growth of the lower-class child, research efforts would have to be focused on the preschool development of such children. Fifth, although the lower-class groups contribute disproportionately to the mentally retarded population, particularly if those with borderline intellectual status are included, it was of theoretical and practical importance to understand why more lower-class individuals were not mentally retarded--a question identical in principle to that raised in regard to the relationship of juvenile delinquency and lower-class status.

It is from the historical perspective briefly described in the previous paragraphs that the present trends should be viewed. In this study, we located a group of preschool children drawn from the lower-class strata of a community and provided them with a preacademic and nursery school experience. In essence, we intervened into the daily lives of these children by involving them in a variety of experiences calculated to engender and reinforce attitudes, motivations and cognitive skills considered prerequisites for normal intellectual and academic growth. In approaching the problem in this way it was our expectation that, in comparison with an appropriate control group, we would find a significantly lower incidence of those intellectual and academic deficiencies so frequently found in this segment of our society.

The Plan of the Book

In the chapter which follows we endeavored to indicate some of the historical background of certain aspects of our research. Those workers who have been in the field for some time will find little new in the review. However, in light of the fact that, in recent years, so many new people have become interested in mental retardation and cultural deprivation, we deemed it necessary that some perspective on our research efforts be given. There is, unfortunately, the tendency in most fields for the younger generation of researchers or practitioners to view those of previous decades as disinterested in or uninteresting about current problems.

Chapter III presents, in some detail, the problems we encountered in locating and selecting subjects, problems that have plagued earlier workers and made research in this area both intriguing and hazardous. Chapter IV describes the manner in which the randomization of subjects into experimental and non-experimental groups was accomplished as well as the psychological and social evaluations which served as the baseline from which subsequent change was analyzed.

Chapters V and VI comprise the most lengthy section of this monograph, perhaps because they deal with the most difficult problems. More specifically, these chapters describe the variety of aspects which characterized the preschool environment into which the experimental subjects were placed. The experiences of these children are not meaningfully communicated by such terms as "curriculum" or "preschool." The school setting is an extraordinarily involved social-psychological unit which at present is difficult to communicate in words, not only because the setting is so complex but also because the methodologies necessary to describe its important aspects are yet to be developed. We can only hope that these chapters give the reader some idea of what was done to and with these children and the spirit which characterized those interactions.

Chapter VII describes the study population on the first testing of May of 1962 and after each subsequent testing. It analyzes the effects of our interventions with respect to between-group variation. More important, we believe, is the consideration given to the correlates of school success insofar as can be determined from the entire population studied.

The last chapter of this monograph summarizes our logistic problems, strategies, and tactics, centering on a discussion of the determinants of school behavior of disadvantaged children.

For the convenience of the reader, and as an attempt to obviate unnecessary discontinuities in the presentation of this research, only the most pertinent data have been presented in the body of this monograph. All other data analyses are located in the Appendix.

Chapter II

Review of the Literature: Educating Intelligence

It is the purpose of this chapter to review and discuss studies concerned with the education of intelligence. Educating intelligence within any segment of our society has numerous significances for all segments. It is the specific intent of this review to focus on two overlapping groups: the mentally retarded and the culturally disadvantaged.

Educating intelligence may be thought of as referring to procedures and conditions that bring out or elicit capacities in the individual for changing, both in rate and complexity, his learning performance insofar as school-related and other problem-solving tasks are concerned. The emphasis here reflects the Latin origin of the word education: to lead forth, to draw forth, bring out, elicit. Change may be measured through the use of intelligence and other standardized and informal tests. On the behavioral level change is reflected in the child's ability to handle with increasing skill the variety of problems confronting him as a student and as a human being. It is our assumption that change becomes both significant, i.e., important, and possible when the individual: (1) needs to change, (2) aspires to change, and (3) is optimistic about the possibility for change. Educating intelligence refers to more than hypothetical "mental faculties or abilities." It also refers to attitudes about self, learning, and abilities without which the phenomenon of change cannot be comprehended. In fact, many of the controversies we discuss in this chapter concern not only the significance of "abilities" but also that of "attitudes" in the learning process.

The literature relevant to the research presented later in this book is vast, partly because it deals with problems as old as man and partly because the questions asked and the answers given were far from clear. It is beyond the scope of this chapter to review that literature in a comprehensive manner. It is our purpose here to give some historical perspective on the problem and, by focusing on certain studies, to indicate what appears to be the status of our knowledge and theorizing and the direction of future research. The reader interested in pursuing the literature in depth is referred to the following: Bloom, Davis, & Hess (1965); Deutsch, et al. (1964); Ginzberg & Bray (1953); Halsey, Floud, & Anderson (1964); Harrington (1964); Hunt (1961); Itard (1962); Jenkins & Paterson (1961); Kirk (1958); Kvaraceus, et al. (1965); May (1964); McCandless (1952); McCullers & Plant (1964); National Society for the Study of Education (1928, 1940); Riessman (1962); Sarason (1959); Schriver (1964); Strom (1965); Warner (1960); and Wellington & Wellington (1965).

Some Historical Trends

The educability of intelligence involves one immediately in conceptions of the nature of man, an involvement which lends fascination to the problem at the same time that it touches off strong feeling and inevitable controversy. It should not be surprising, therefore, that even in the research literature cold data and hot controversy have existed side by side. In no aspect of the problem of the educability of intelligence is this better seen than in the trends discernible in the heredity-environment, or nature-nurture, polarity.

The earliest trend is characterized by the first important written record of a systematic attempt by man to educate the intelligence of a retarded child. In their introduction to The Wild Boy of Aveyron by Itard, first published as memoirs in 1801 and 1806, the English translators, George & Muriel Humphrey (1962), described the tenor of the times immediately following the French

Revolution.

The scientific discoveries of Benjamin Franklin, Lavoisier and Galvani made it appear as if ". . . nothing was impossible to science." The enlightened philosophies of Voltaire, Montesquieu, and Rousseau extolled the nobility of all mankind. There was hope, for the first time, that the deaf would speak and the blind would circumvent their handicap. The curability of mental illness was not thought impossible. Certainly, these were days for great dreamers and the period was ready to hear a young doctor say that, with a civilizing experience, a retarded boy will become normal. In 1799 Victor, the Wild Boy of Aveyron, came to the attention of a physician, Jean-Marc Gaspard Itard, after the boy was seized in the forests of Aveyron and brought back to Paris for study and observation. He was about eleven or twelve years of age, found completely naked with 26 scars over his face and body. Was this Rousseau's "noble savage" or was this some idiot left to perish in the woods by an unfeeling family? The boy squatted for hours on the ground behind some shelter. He would pay no attention to any movement about him; rain and cold were alike to him; he was indifferent to filth. When he stood or sat, he rocked back and forth like some wild animal. He had no speech and did not appear to hear. Pinel, a leading authority of the day, diagnosed the boy as an incurable idiot. However, Itard disagreed and requested and received permission to take Victor under his care for training. He believed that Victor's apparent retardation was due to his general inexperience and lack of training, central to the normal development of any civilized person. For four years Itard lavished his skills and affections on the young boy. Through the bombardment of a variety of sensory stimuli, from the gross to the finely discriminating, Itard was able to accomplish a great deal with him. Victor became "human-like." He

He learned to recognize letters, to arrange them in words, to form sentences, and to write. However, the training of Victor's auditory sense became an almost impossible matter. The boy was not deaf--if the key of his door was so much as touched, he drew himself ready to escape--yet he could not learn to speak more than a few words. After repeated discouragements, Itard finally gave up his work with Victor. Later, at the request of the French Academy, he published his memoirs and here lies the first recorded efforts to educate the intelligence of a defective child. Whether this was an individual born deficient or psychotic (Silberstein & Irwin, 1962) or, as Itard believed, a child made deficient by an adverse environment, is less important than the evidence that the intellectual functioning of a seemingly defective human being was improved through educational intervention. Itard's work is an interesting document of the philosophy of early "sensationalists." He deserves credit as the scientific progenitor of such pioneers in the field as Seguin and Montessori. Two analyses of his work can be found in Sarason (1959, pp. 321-330) and Kirk & Johnson (1951, pp. 70-74). In addition, Kirk & Johnson (1951, Chapter 4) and Kolstoe (1956) provided us with interesting accounts of the contributions of Itard as well as other sensationalists of this period and their influence on modern special education practice with the mentally retarded.

The next trend can be described as the "Measurement Era." Begun with the early work on hereditary genius by Francis Galton (Jenkins & Paterson, 1961), it reached its zenith with publication of Alfred Binet's historic research on measuring intelligence. The work of these and other early pioneers gave rise to Goddard's study of the hereditary nature of feeble-mindedness (1912). His publication, The Kallikak Family, evoked wide discussion, particularly because of his conclusion that cultural-familial mental retardation was transmitted through a multiple genetic mechanism. His book had an effect upon the field of mental retardation that lingers today. Through tracing the illegitimate progeny of Martin Kallikak and contrasting this with his legitimate progeny, Goddard concluded that mental retardation is inherited in 65 per cent to 90 per cent of cases. Although Goddard's work was more or less rejected by the scientific community scarcely ten years after its publication, the combination of the early American testing movement and works such as Goddard's resulted in our strongest nativist era. To be sure, during the first fifty years of this century there were scientists reporting studies that questioned the inheritability of cultural-familial mental retardation. However, the works of Woodworth, Boring, and Alexander (Jenkins & Paterson, 1961) and the very influential investigations of Skeels & Dye (1939), Skeels & Harms (1948), and Skodak & Skeels (1949), are less characteristic of the period than the reports of Thorndike, Lahey, and Gesell and other so-called nativists (Jenkins & Patterson, 1961). In fact, several recent works on mental retardation continue to reflect the nativist period of our first half-century (Jervis, 1954; Michal-Smith, 1956; Tarjan, 1959; Wallin, 1956; and Yannet, 1957).

Ironically, Alfred Binet, probably the most influential scientist in the resurgence of nativist philosophy of the early twentieth century, was not certain of the variables involved in developing intelligence. His position is surprisingly similar to that of modern environmentalists:

> Our purpose is to be able to measure the intellectual capacity of a child who is brought to us in order to know whether he is normal or retarded. We should therefore, study his condition at the time and that only. We have nothing to do either with his past history or with his future; consequently we shall neglect his etiology, and we shall make no attempt to distinguish between acquired and congenital idiocy; for a stronger reason we shall set aside all consideration of pathological anatomy which might explain his intellectual deficiency. So much for his

> past. As to that which concerns his future, we shall exercise the same abstinence; we do not attempt to establish or prepare a prognosis and we leave unanswered the question of whether this retardation is curable, or even improvable. We shall limit ourselves to ascertaining the truth in regard to his present mental state (Jenkins & Paterson, 1961, p. 90).

The third, very brief but significant, movement began shortly after World War II with publication of the Schmidt study of changes in the behavior of children originally classified as feeble-minded (1946, 1947). With the advent of a powerful parent movement, with reports of new drugs and surgical techniques presumably to cure retardation, with the beginning of widespread acceptance of special class education for trainable children and sheltered workshop and other care programs for severely retarded children and adults, the public was eager to accept studies that aimed to prevent or reverse mental retardation. Retarded children could be helped and again, for awhile, it seemed as if anything was possible. Schmidt, whose study began in Chicago in 1935, investigated changes in the behavior of children participating in an especially planned program and originally classified as "feeble-minded." The general objectives of the special education program described in her study were: to decrease nervous tensions, remove emotional blocks, and develop social interaction, self-confidence and self-worth. She reported unusually high IQ gains for the experimental group in addition to concomitant increments in social and vocational adjustment; insignificant gains were reported for the control (conventional special class) group.

Soon after the publication of Schmidt's study, Kirk (1948) disputed her findings on the following bases: at the time, the original IQ scores reported by Schmidt appeared much lower than those found in special classes in the Chicago Public Schools; Schmidt administered the Bernreuter Personality

Inventory to subjects in spite of the fact that, to adequately take this test, one needs at least high school reading ability; statistical errors were found; her original data were not available for inspection. In Schmidt's reply to Kirk's evaluation of the study (1948), she did not offer strong refutation to criticisms of her research.

Kirk's dismissal of Schmidt's research, as well as the several other equally negative reviews (Hill, 1948; Nolan et al., 1949; Stevens, 1948), may have accentuated a climate of thinking not conducive to systematic research on the educability of intelligence. It seemed as if the attempt to bring about or to understand increments in intellectual development was not particularly fashionable. Interestingly, the individual most responsible for the derogation of Schmidt's study, Kirk, was one of the few researchers both interested and sufficiently optimistic to investigate, later, the effects of various environments on the intellectual and social competency of mentally retarded children (1958). In effect, however, the work of Schmidt, so highly publicized and so hopeful, resulted in deeper entrenchment of nativist philosophy among many educators and a reluctance to study possibilities for educating intelligence.

But for the brief interruption by the "Schmidt Era," the decade following World War II was marked by a rather strong nativist philosophy (Goldstein, 1948; Blatt, 1960, 1961). Slobody's chapter in Michal-Smith's text (1956), voiced the conventional viewpoint in regard to familial mental retardation:

> Previously, this category was considered as representative of the largest percentage of the mentally retarded population. It is believed that this form of mental retardation is produced by the transmission of multiple abnormal genes from defective parents. At one time, this was considered to be purely a functional disturbance; however, recent studies

> have shown various significant pathologic abnormalities in the central nervous system. Although it has been shown that there is a greater incidence of abnormal electroencephalographic records in this group, only a slightly higher incidence of convulsive disorders is seen, as compared with the general population. The diagnosis is made in the absence, either in the history or the physical examination, of any causative factor for mental retardation in a child, other than the presence of defective or inferior intelligence in siblings and in one or both parents. Undoubtedly, many instances of mental retardation have been placed in this category erroneously because of the limited means available to diagnose organic cerebral abnormalities accurately (pp. 34-35).

An indication of the attitude embraced by many special educators and others working with the mentally retarded during the decade following World War II can be found in reviewing Behavioral Research on Exceptional Children, edited by Kirk & Weiner (1963). Of the seven studies reviewed by Heber on the influence of environmental factors on intellectual development, no more than one, Pasamanick's, had its origin during that decade, and this study did not deal specifically with retarded children. The other studies were begun either prior to World War II (although some were actually reported during post war days) or very recently.

The most recent discernible trend owes its emergence to several important occurrences. The first of these was the monograph by Sarason & Gladwin (In Masland, Sarason & Gladwin, Mental Subnormality. New York: Basic Books, 1958, pp. 145-442.), an extension of Sarason's original work in 1949. This monograph dealt, probably for the first time on a systematic and comprehensive level, with psychological and cultural problems in mental retardation. Perhaps the major contribution of this monograph was its attempt to describe and discuss the numerous factors which had to be considered in research relevant to heredity-environment positions. Although their work may not have resolved many issues, it did indicate that earlier formulations of the problems, as well as interpretations of previous research, left much to be desired.

The Sarason-Gladwin monograph was followed by the Manual on Terminology and Classification in Mental Retardation of the American Association on Mental Deficiency (Heber, 1959). This manual contained some marked changes over earlier ones, particularly in its attempt to define mental retardation in terms of functioning level rather than in terms which prejudged etiological determination and prognosis. The new definition referred to function rather than, as was traditional, to capacity; it did not require a prognosis of retardation at maturity and, consequently, left open the question of reversibility of condition; the definition did not assume a constitutional condition of the central nervous system to be present in all cases of mental retardation; and it required more than the IQ score as evidence of state of intellectual functioning.

A third, and perhaps most significant, factor that gave substance to the present era of thinking was the involvement of the Federal Government, in general, and former President Kennedy, in particular, in providing stimulation for the development of research and service programs to aid all handicapped children. The report of the President's Panel on Mental Retardation, National Action to Combat Mental Retardation (1962), contained the following:

> The majority of the mentally retarded are the children of the more disadvantaged classes of our society. This extraordinarily heavy prevalence in certain deprived population groups suggests a major causative role, in some way not yet fully delineated, for adverse social, economic, and cultural factors. These conditions may not only mean absence of the physical necessities of life, but the lack of opportunity and motivation. A number of experiments with the education of presumably retarded children from slum neighborhoods strongly suggests that a predominant cause of mental retardation may be the lack of learning opportunities or absence of "intellectual vitamins" under these adverse environmental conditions. Deprivation in childhood of opportunities for learning intellectual skills, childhood emotional disorders which interfere with learning, or obscure motivational factors appear somehow to

> stunt young people intellectually during their developmental period. Whether the causes of retardation in a specific individual may turn out to be biomedical or environmental in character, there is highly suggestive evidence that the root causes of a great part of the problem of mental retardation are to be found in bad social economic conditions as they affect individuals and families, and that correction of these fundamental conditions is necessary to prevent mental retardation successfully on a truly significant scale (pp. 8-9).

Insofar as research is concerned the report stated:

> Research in the behavioral sciences is at present primarily addressed to therapeutic and rehabilitative possibilities. The most fertile unploughed area for further behavioral and social science research is indicated by the accumulating evidence that a host of social, economic, and environmental factors--often categorized as cultural deprivation--are correlated or associated to a high degree with the incidence of mental retardation, especially in its milder manifestations of low intellectual and social performance (p. 24).

The Panel specifically went on to recommend high priority for the development of research centers to study psychological and cultural factors relating to the etiology of mental retardation. However, perhaps more than the great sums of money and services now offered by the Federal Government to combat mental retardation, the message to the 88th Congress by former President Kennedy calling for a total national program was of major importance.

> Cultural and educational deprivation resulting in mental retardation can also be prevented. Studies have demonstrated that large numbers of children in urban and rural slums, including preschool children, lack the stimulus necessary for proper development in their intelligence. Even when there is no organic impairment, prolonged neglect and a lack of stimulus and opportunity for learning can result in the failure of young minds to develop. Other studies have shown that, if proper opportunities for learning are provided early enough, many of these deprived children can and will learn and achieve as much as children from more favored neighborhoods. This self-perpetuating intellectual blight should not be allowed to continue (1963, p. 10).

The above quotation can be regarded as a kind of lineal descendant of

Itard's writings. We have come, so to speak, full circle in that we seem to be in a period characterized by optimism about what can be accomplished with certain groups among the mentally retarded. Optimism, however, lends no validity to ideas and should not be substituted for the many studies which will have to be carried out before we understand sufficiently the complicated interactions between heredity and environment. That this optimism is powered by the money necessary to do such studies represents "a first" in the history of mental retardation and gives hope that substantial scientific progress will serve as a basis for discarding erroneous ideas and faulty formulations.

Cultural-Familial Retardation

Although, over the years, the heredity-environment controversy affected discussion and study of the many and radically different types of conditions (glandular, metabolic, degenerative, brain disfunction, etc.), there is no doubt that the cultural-familial mentally retarded was the group around which the controversy was most heated. This group represented the largest sub-group among the mentally retarded and, in addition, confronted our society with numerous problems of social policy and action. Those with different concepts of its etiology could not be expected to react dispassionately. Differences in conceptions of etiology made vast differences in what could or might be done to reduce the incidence and consequences of the condition.

As might be expected, the cultural-familial retarded group was not exempt from the changes in viewpoint briefly described in the previous section. This change is most clearly seen in a comparison of the Etiological Classification Manual of the American Association on Mental Deficiency (1957) with the Manual on Terminology and Classification in Mental Retardation of that same

Association, published two years later (1959). In 1957, the Association had the following to report on the familial etiology:

> This category depends on multiple causative mechanisms of which the most distinctive is an inherited sub-average intellectual status or inadequacy. All the evidence tends to indicate that the genetic mechanism is polygenic, and represents either in a qualitative or quantitative sense, and accumulation of those items of the polygenic "intelligence" transmitting factor which determines the lower part of the normal distribution curve for intellectual capacity. In other words, we are dealing here with "normal" or physiological genes involved in the inheritance of intelligence. It differs from other hereditary conditions associated with mental retardation in that the latter represents, as a rule, clearly abnormal or pathological genetic factors, arising originally through mutations, and not present in the normal population, genetically speaking (p. 14).

The 1959 Manual placed cultural-familial mental retardation under the category "Mental Retardation Due to Uncertain (or Presumed Psychologic) Cause with the Functional Reaction Alone Manifest":

> In addition to absence of reasonable indication of cerebral pathology, classification in this category requires that there be evidence of retardation in intellectual functioning in at least one of the parents and in one or more of the siblings where there are such.
>
> Because of the parental inadequacy in these cases there is usually some degree of cultural deprivation present. This deprivation is not generally of such a severe nature as to warrant classification under psychogenic mental retardation associated with deprivation of stimulation. In those cases where the cultural deprivation is of severe degree, classification under cultural-familial mental retardation takes precedence where there is a familial history of intellectual subnormality.
>
> There is no intent in this category to specify either the independent action of, or the relationship between, genetic and cultural factors in the etiology of cultural-familial mental retardation. The exact role of genetic factors cannot be specified since the nature and mode of transmission of genetic aspects of intelligence is not yet understood. Similarly, there is no clear understanding of the specific manner in which environmental factors operate to modify intellectual functioning.
>
> Cultural-familial mental retardates invariably exhibit a mild degree of retardation in measured intelligence and adaptive behavior (pp. 39-40).

From the above it appears that the more or less accepted viewpoint had been altered and the condition was now predicated upon the following: (1) an absence of demonstrable central nervous system pathology, or a type of minimal pathology not considered of etiological significance as far as mental retardation is concerned; (2) intellectual functioning within the retarded range in at least one of the parents and one or more siblings where there are such; (3) retardation of a mild degree; (4) usually of a lower socio-economic class; (5) the use of the term, cultural-familial mental retardation, does not presuppose that the condition is inherited through some multiple or other genetic phenomena; nor does it presuppose that it is not. At this point, the question appears to be regarded as an open one, a position which we fully share.

The fact that the question of etiology may be regarded as an open one should not obscure what appears to be near unanimity of opinion that the biological substratum of intelligence ultimately will be found to reflect genetic mechanisms and processes. There is no reason to assume that the human brain is exempt from the influences contained within the genetic material from which the human individual develops. However, the relationships between brain and intelligence are so little understood, and so poorly studied, that many investigators consider it unwarranted to implicate genetic mechanisms at this time. This position is exemplified in the following statement by Sarason & Gladwin:

> . . . it will be our thesis that a hereditary determinant of mental capacity must not be assumed to exist unless proven. Furthermore, proof should be sought in terms of our present knowledge of human genetics and the nature of human intellect, rather than, as is commonly done, through the administration of routine intelligence tests to a variety of different 'racial' and other groups. We do not propose to deny that heredity is a factor, particularly in mental deficiency, but rather that we should leave it out of our accounting until it is supported by more than

speculation and bias (Sarason, 1959, p. 448).

It seems appropriate at this time to present some conclusions from the research literature which illustrate not only the difficulties in interpretation of findings for the question of etiology but also the kinds of problems which make research in this area so thorny.

As the diagnostic label suggests, cultural-familial retardation is a phenomenon of the lower end of the social class continuum. Characterizing this group as "Kallikak" or "sub-cultural" reflects the observation that these cases come from a segment of our society considered markedly different from the so-called middle-class structure. The Onondaga study (New York State Department of Mental Hygiene, 1955), the Fouracre, Rooke, & Botwin survey (1961) and the Kennedy, Van DeRiet, & White monograph (1961) reported extraordinarily high incidences (up to 40 per cent) of mental retardation in neighborhoods that are non-white and/or socio-economically deprived. These and other surveys, however, differ markedly in their incidence statistics probably because of differences in neighborhoods surveyed, age of groups surveyed, and the criteria of mental retardation employed. The importance of determining the significance of these differences resides in its relevance for the following questions: Do these differences in incidence reflect differences in the degree of cultural impoverishment and social disorganization and, if so, how can one identify social variables which have the most significant and direct impact on the developing child? If incidence figures truly vary with the age of the groups studied, how can this be accounted for?

A second problem involves a question which current surveys have not answered and, indeed, by their nature may be unable to answer and yet is crucial for understanding cultural-familial mental retardation. Put in its most extreme form the question is why are not all children in the most culturally deprived or socially disorganized neighborhoods retarded? How may we account for the "successful" slum child? Another form of the question is why are not all children in a Kallikak-type family retarded? These questions are identical,in principle,to those asked in relation to problems in juvenile delinquency. They, in principle, pose no problem for those who emphasize the role of heredity but neither do they pose problems for the environmentalist who assumes that two children in the same family must experience their environment in very different ways.

A third problem in dealing with an understanding of cultural-familial retardation is suggested from the literature on the post-school behavior of the mentally retarded. The problem involves the relationship between school problem solving behavior and non-school problem solving behavior of the cultural-familial group (Blatt, 1961a). The literature indicates (Sarason, 1959) that mildly retarded adults--the bulk of whom are in the cultural-familial category--are, in general, indistinguishable from other members of their cultural milieu. They maintain themselves independently in society, marry, and find jobs with or without the benefit of special help. A review of follow-up studies of the mildly retarded by Abel(1940), Baller (1936), Bobroff (1956a, 1956b), Charles (1953), Dinger (1961), Dunlop (1935), Fairbank (1933), Hegge (1944), Kellogg (1941), Kingsley & Hyde (1945), McIntosh (1949), McKeon (1946, 1948) and Muench (1944) revealed that school tests of problem solving behavior do not adequately predict non-school problem solving behavior, i.e., the group known as mentally retarded had demonstrated a greater degree of out-of-school success, both socially and vocationally, as compared with performance in school and predictions based upon psychological tests. This type of finding is but another basis for questioning the soundness of attempts to understand the etiology of cultural-familial mental retardation by depending solely on intelligence test data.

Cultural Deprivation

In earlier decades, particularly when the nativist or hereditarian point of view was dominant, the cultural-familial type of mental retardation was viewed apart from the even larger group in our society variously labelled as the disadvantaged or the culturally deprived. This separation appears to be breaking down, the important study by Ginzberg & Bray (1953) on The Uneducated being but one example demonstrating close relationships among illiteracy, educational failure, mental retardation, and cultural factors. Aside from the significance of the concept of cultural deprivation for the research to be presented in later chapters, there are three reasons why some aspects of the literature on cultural deprivation should be presented at this point. First, this literature contains some fascinating examples of the consequences of attempts to intrude in pervasive ways into the lives of culturally disadvantaged individuals, attempts similar to those in our own research. Second, this literature underlines the caution that prejudging the capacities of individuals to change can result in action (or inaction) which "proves" the prejudgments. Third, and as important as what has so far been said, prior methodologies and findings do not permit one to draw unassailable conclusions. At the very least, however, the emergence of the seriousness of cultural deprivation into the national consciousness has markedly widened the scope of the formulations relevant to cultural-familial mental retardation, in particular, and cultural deprivation, in general.

National concern for the problem presented by cultural deprivation in our communities, especially in large urban centers, and the resulting problems of school drop-outs, delinquency, academic failure, and retardation is reflected in such new nation-wide studies as the National Education Association Project on School Drop-outs, the ever increasing local projects to combat the problem, and the recent flood of literature on the culturally deprived (also called "culturally disadvantaged," "educationally disadvantaged," "underprivileged," or "poor".) Riessman's recent book The Culturally Deprived Child (1962) and Della-Dora's (1962, 1963) discussion of the implications of cultural deprivation for education presented a picture of this group that is not discrepant with the characterization of the cultural-familial retarded discussed earlier: (1) by traditional methods of evaluation their intelligence is often retarded; (2) the intellectual level and social adequacy of the parents appear to be more or less retarded; (3) there is no discernible central nervous system pathology in either children or parents; (4) they are born into and reared in a cultural milieu which is "inferior" to other strata of our society; and (5) they represent a disproportionately large part of the case load of many of the social agencies.

Although there are dozens of programs now being conducted nationally with the culturally deprived, the Higher Horizons Project of New York City was a prototype and has been extensively discussed. As descriptions and critical evaluations of Higher Horizons can be found in the literature (Riessman, Chapter 11; Mayer, 1961, Chapter 7, and Della-Dora), we will but briefly note here its aims and accomplishments. In 1956, at Junior High School 43 in New York City, this project was initiated in order to identify, encourage, and prepare for college those students coming from low socio-economic homes who would otherwise have neither the financial backing in or the preparation to consider this aspiration. Junior High School 43, located in the heart of a Manhattan slum area, had a student body with an average IQ of about 80 and very high truancy. Fewer than 40 per cent of its graduates went on to graduate from high school. The specific objective of the project was deceptively simple: to convince children that they could achieve. The project emphasized: special remedial classes; intensive counseling; parent involvement; extensive after-school use of educational facilities; extensive cultural programs in music, art, theater, and literature; visits to industrial and cultural centers both in New York and out of the city--all for the purpose of expanding cultural and intellectual horizons of the students and in convincing them that they had heretofore undeveloped abilities. How did the project fare? Attendance figures went up remarkably in contrast with pre-project statistics; most students were entering and graduating from high schools (including some of the city's finest, e.g., Bronx Science, Brooklyn Tech, Stuyvesant, Music and Arts;) more than half of the students showed significant gains in IQ; more students eventually entered colleges and universities.

However, one cannot take lightly the criticisms Riessman made in his evaluation of Higher Horizons (1962):

> There is no question that the Program did a splendid job in demonstrating conclusively that educationally deprived children can learn. The point at issue is whether the Program itself produced this learning. Is it possible that the achievements did not come about from the announced methods of the Project, but are a by-product of the experiment itself? What does this mean? In order to consider this possibility it will be necessary to go back to a now-famous social science investigation.
>
> Some years ago a classic experiment took place at Western Electric that discovered something which has come to be known as the "Hawthorn effect." Here, factory workers' production and morale were greatly enhanced by putting them in special groups and varying the lighting in

> the rooms. At first, the results seemed obvious, because with better lighting, production went up. But then it was discovered that similar increases in production occurred when the lighting was decreased! Apparently, the very setting up of special experimental groups, and the concomitant attention, was sufficient to produce the observed results. This kind of placebo effect is similar to that found in modern medicine, where people appear to be cured by some drug, while actually the simple taking of a pill containing no drug is often sufficient to produce the same effect.
>
> One can only wonder whether a similar process is at work in the Higher Horizons success story. After all, a great deal of excitement was engendered by the newness of the experiment, the positive democratic goals, the increased input of effort and resources. Any one, or all, of these factors may have stimulated enough enthusiasm to achieve the obtained results, independent of the specific methodology employed. In addition, another variable may have been operative in this situation. Deprived children have been notoriously neglected by the school system, and perhaps the very fact of their neglect has been the decisive one in their hitherto poor performances. Higher Horizons came along, and quite apart from its specific approaches, the underprivileged children were given a great deal of attention. Conceivably, this may have been enough to produce the findings (pp. 103-104).

Reissman further made the point that the nature of the study precluded ascertaining which of its aspects contributed in what degree to the findings: smaller classes, carefully picked teachers, involvement of parents, numerous trips, etc.

> It might be objected, "Who cares which variables were decisive; the important thing is they got the results." Unfortunately, the naive pragmatism underlying this defense is not even good pragmatism. We need to develop approaches that will be effective on a large scale in the everyday school setting, where teachers are not working day and night and 14 Sundays per term! We need techniques that can be applied by the average teacher, hopefully with a fair amount of devotion, but not necessarily the short-lived zeal fostered by a unique experiment. There are an increasing number of reports that as the Project has spread, the enthusiasm of the overworked teachers has begun to wane (p. 104).

It is extremely important to emphasize that a carefully documented description and evaluation of the Higher Horizons Project has not yet appeared. The great publicity given to this project should give little comfort to those concerned in separating wish from fulfillment. We would agree with Reissman's conclusion that ". . . the Program does demonstrate that the culturally deprived can be educated, and this is an extremely important service in the age of non-belief." (p. 111). We also agree with the implication of his criticisms that many more studies need completion before we comprehend and can control the most important factors involved in educating the intelligence of the culturally deprived.

Changes in Intellectual Functioning

There have been numerous studies or reports bearing on the problem of change in level of tested intelligence, some more and others less directly relevant to cultural-familial mental retardation and cultural deprivation. Because the bulk of these studies are retrospective (i.e., they were not planned interventions) we shall focus mainly on those which, as in the case of our own research to be presented later, are prospective in nature and involve some form of planned intervention or environmental manipulation. We do not say that the retrospective type of study is less important than the prospective type, but rather that our own research may be seen in better perspective by discussing those earlier studies to which it is related. Put in another way: the thorny problems of methodology, subject selection, statistical analysis, and sources of bias which confronted us in our research must be viewed in terms of earlier and similar types of studies. The retrospective type of study has been described and evaluated in a number of reports: Skeels & Dye (1939), Skeels & Harms (1948), Skodak & Skeels (1949), DeGroot (1948), Lorge (1945), Martz (1945), Sarason (1959), Masland, Sarason, & Gladwin (1958), and the studies of various researchers brought together in the book edited by Jenkins & Paterson (1961).

That changes in tested intelligence occur with sufficient frequency so as to be considered an accepted--and not wholly unexpected--metric phenomenon is hardly debatable. The Twenty-Seventh (1928) and Thirty-Ninth (1940) Yearbooks of the National Society for the Study of Education, Windle's recent monograph (1962), Yarrow's review of research on maternal deprivation (1961) and Pinneau's Changes in Intelligence Quotients (1961), a longitudinal report of the Berkeley Growth Study, are sufficient documentation insofar as the inconsistency of the IQ is concerned and are suggestive of conditions that give rise both to increments and decrements in tested intelligence.

These changes are said by some to be illusory, not "actual changes" in the individual but ones caused or partly explained by insensitive psychometric tools, poor test administration techniques, disturbed subjects during time of test, or poor rapport between subject and tester. There can be little doubt that these represent relevant criticisms of many studies, although they do not represent an explanation of all findings involving changes in intellectual functioning concomitant with defective or beneficient environments. These criticisms also are inadequate to explain the many studies demonstrating changes in test scores as a function of personality variables.

The early work of Skeels and his associates is a splendid example of a retrospective study arising as a result of service responsibilities, and culminating in a prospective study. In 1939 Skeels & Dye reported observations bearing on the effects of differential stimulation on mentally retarded children. Their study had its genesis with the rather surprising discovery that after placement of two eighteen-month-old children, both moderately to severely retarded, in an institution for the feeble-minded, their IQs went up very dramatically. These two children, with original IQs of 46 and 35, and qualitative observational evidence substantiating these psychometric findings, born illegitimately of feeble-minded mothers, were committed to a ward of older girls, ranging in age from 18 to 50 years. After six months at the institution for the retarded, the children had IQs of 77 and 87 on the Kuhlmann-Binet. A year later they obtained IQs of 100 and 88. At chronological ages 40 months and 43 months respectively, their IQs were 95 and 93. The investigators were very puzzled and interested in this unusual development and very carefully studied the institutional environment. They found that the attendants on the ward and the other patients took a great interest in their "babies." On days off, attendants took these children with them for car rides or to town on shopping trips. They brought them toys, books and play materials. The older female residents would play with them and take them for walks. Eventually, as justification for continuing these children in an institution for the retarded became very questionable, they were placed in foster homes. After about fifteen months in the new placements, their IQs were measured at 94 and 93.

As a result of this experience, Skeels and his associates convinced the State Board of Control to approve the informal transfer of one and two-year-old mentally retarded children from the state orphanage nursery to the state school for the retarded. A contrast population, not designated as a group until the close of the experimental period, were continued in the orphanage. It was the purpose of this study to determine the effects on the mental growth of these children of the radical shift from one institutional environment to another. The experimental group, 13 in number, from one to two years of age, were placed singly or by two's with brighter and older girls at the state institution for the retarded. Their mean IQ at the time of transfer was 64.3; the contrast group, 12 in number, with a mean IQ of 86.7 remained at the orphanage. Over a two-year period, the experimental group made an average IQ gain of 27.5

points while the contrast group showed a loss of 26.2 points. Skeels & Dye concluded that a change from retarded to normal intelligence in preschool children may be possible, in the absence of organic pathology, by providing the child with a more adequate psychological environment. Conversely, they found that children of typical intelligence can become retarded under a continued adverse non-stimulating environment. Their later studies, investigating the mental development of children from inferior social and intellectual backgrounds who were placed in foster homes during infancy, more or less substantiated the above findings, i.e., these children attained levels of intellectual performance that were consistently greater than the predicted performance inferred from the characteristics of the true parents (Skeels & Harms, 1948; Skodak & Skeels, 1949).

That the findings were plausible and gave a foundation for optimism should not obscure those factors which did not permit one to consider the above study a definitive one--factors which, by their number and nature, make it extraordinarily difficult for any one study to resolve all of them satisfactorily. First, one cannot be sure that the selection of subjects for the experimental and contrast groups did not reflect unwitting bias that influenced the direction of the findings. If subjects had been randomly placed into the two groups, one could at least be assured that no conscious or unconscious experimenter bias was operative. Second, the number of subjects was small, a factor frequently beyond the control or means of the researcher but which, unfortunately, can contraindicate the use of randomization where more than one variable is being considered. Third, unless one was sure that appropriate measures were taken to control for bias on the part of those administering the psychological tests, it is not unreasonable to contend that tester bias may have selectively contributed to the findings. This third factor, it should be noted, is in practice more difficult to control than is ordinarily recognized, involving as it does the problem of equating examiners for experience and personality, the problem of obtaining examiners who are truly ignorant of the purposes of the study, and who do not have differential conceptions about the groups involved, e.g., institutional vs. non-institutional children. Fourth, it is by no means clear how the two environments differed and which differences were the most influential, a point well made by Riessman in connection with the Higher Horizons Project discussed in the previous section. Fifth, since the experimental and contrast groups differed initially in test score, the subsequent differences may reflect, to an undetermined degree, statistical regression, a point which will be elaborated upon later.

Two of the numerous Iowa studies (Coffey & Wellman, 1936; Skeels, Updegraff, Wellman, & Williams,1938) investigated children of preschool ages from varying social backgrounds--those with parents in the professions to those who were institutionalized in orphanages--and found that children who attended nursery school (Coffey & Wellman) showed gains in IQ and those children who did not attend nursery schools (Skeels, et al.) showed decreases in IQ. Goodenough, whose own research did not find the consistent changes in intelligence reported during the 1930's and 1940's by the Iowa Child Welfare Research Station (Goodenough & Maurer in Jenkins & Paterson's Studies in Individual Differences, 1961, pp. 504-511), was a participant in a heated debate on the so-called nature-nurture controversy of the period. Goodenough & Maurer criticized the small number of cases in most studies of this type, uncontrollability of such factors as differential acquaintance with the examiners or test items, and bias of examiners who knew to which group the various children belonged and, in fact, knew particular children. However, Goodenough & Maurer's most devastating criticism--quite well taken--concerned itself with the the phenomenon of regression in psychometrics:

> This study is merely a concrete illustration of the misleading conclusions that have resulted from a statistical practice that was begun in Wellman's 1932 study and which the Iowa authors continued to employ in practically all their investigations in spite of the fact that its mathematical indefensibility has been repeatedly pointed out. The procedure consists of classifying subjects on the basis of intelligence quotients earned on the first test given and computing the mean change in intelligence quotient from initial to final testing for each of these groups separately. It is obvious that when this is done, statistical regression due to errors of measurement renders it mathematically certain that unless other factors are operating to obscure the results, the cases originally testing high will appear to lose and those originally testing low will appear to gain, since, owing to the fallibility of the measuring instrument, chance as well as true ability will play a part in determining the original grouping. When the chance errors are reassorted at the time of the second test, each group will 'regress' toward its own true mean with the result that those initially at the upper extremes, whose position was determined in part by real ability and in part by good luck, will appear to lose while those who, for analagous reasons, were initially at the lower extreme will appear to gain. The amount of this regressive gain or loss will be the algebraic mean of the chance error for each group. Because the element of chance plays a much greater part in the mental test scores of young children than of older ones, the magnitude of the regressive shift at the early ages will be correspondingly large. If, moreover, as frequently happens in the case of young children, there is a general tendency toward better rapport at the time of the final than at the time of the initial test, with the result that the final mean of the entire group is shifted upward, the regressive "losses" of the upper group may be largely or wholly masked. Their IQs will then show little change while the "gain" of the lower groups will be much increased, since the regressive shift is always toward the mean of the second measurement (Jenkins & Paterson, pp. 505-506).

In order to demonstrate their point, Goodenough & Maurer discussed data from their own University of Minnesota Nursery School studies, recomputing their findings according to the Iowa pattern and obtaining results not substantially different from those reported from Iowa. Goodenough & Maurer's conclusion was, ". . . the Iowa statistical laboratory has played a far greater part in effecting the 'intelligence' of children than has the Iowa nursery school, and that the differential pattern of gains and losses upon retest shown by children whose initial IQs fell at the extremes of the distribution is a statistical rather than an educational phenomenon (Jenkins & Paterson, p. 511)."

As mentioned earlier in this chapter, except for the Schmidt study and the subsequent critical publications on it, during and after World War II there was little or no interest in early education studies with the mentally retarded in general and the cultural-familial group in particular. In part this reflected a reaction against the Schmidt and Iowa studies, in part the almost total lack of university research centers in mental retardation, and in part a lack of national awareness about the significances of the problem of cultural deprivation. This situation changed markedly in the last decade as may be seen by the studies of Kirk (1958) and Fouracre, Connor,& Goldberg (1962). Because Kirk's study is the more relevant to our own research we shall center our attention on it.

It was the purpose of Kirk's study to answer these questions:

1. Does preschool training of mentally retarded children displace the rate of development of such children as compared to children who do not obtain the benefits of early training?
2. Does the rate of growth of the preschool age continue at an accelerated rate, or does it return to the original rate of development during the primary school years?
3. Are the results similar for children living in different environments, such as their own homes, foster homes, or institutions for the mentally deficient?
4. Are there differences in the changes in rate of growth as a result of training between children whose retardation is ascribed to organic factors and those whose retardation is ascribed to cultural or environmental ones? (p. 9)

Kirk's study, once again opening for investigation the conscientious nature-nurture controversy, identified, evaluated and followed 81 mentally retarded children between the ages of three and six during a three-to-five-year period. Twenty-eight children comprised the "community experimental group, "i.e., children attending Kirk's experimental preschool in the community and followed up from one-to-four years after leaving the preschool. Fifteen children who had been committed to an institution for the retarded were placed in an institutional preschool and later followed after discharge from the preschool, either to the institutional school or to the community. Kirk called this group the "Institution Experimental Group." The "Community Contrast Group" comprised 26 children, living in the community and not attending any preschool. The "Institution Contrast Group" consisted of 12 children, institutionalized in a school for the retarded and not attending any preschool. With few exceptions, the children in all four groups had IQs between 45 and 80, had been examined at the beginning of the experiment and diagnosed as mentally retarded, and had been regularly followed throughout the experimental and post-experimental years.

Kirk and his colleagues found great hardship in locating both community and institutional preschool mildly retarded children. They obtained names of possible candidates from social workers, public health nurses, physicians, and school officials. Referred children from the community cases were, for the most part, either not retarded or severely retarded. Sufficient numbers of community children were eventually found by sending psychologists to lower socio-economic areas simply to knock on doors in order to solicit cooperation from families of possible candidates. The research staff had equal difficulty in locating suitable institutional cases and finally had to go to a second institution in order to find a dozen children to serve as the contrast group. This inability to locate preschool mildly retarded children, led Kirk to suggest that:

> . . . many children later placed in special classes or institutions are not mentally retarded in terms of intelligence test scores at the ages of three, four, or five. Some children whose older brothers and sisters were in special classes, tested approximately normal at the preschool ages. This raises the question as to whether children from low cultural levels who are approximately normal at an early age may later become mentally retarded because of their cultural environment or other unknown variables (pp. 692-700, 1952).

In general, Kirk found that preschool education had some favorable effects on the development of mentally retarded children. His major findings disclosed that, of the 43 children who were in these preschool programs, 30 showed an acceleration in growth during the preschool and held that level during the follow-up years, as described in the case study data presented in his monograph; there was greater difficulty in displacing the rate of growth of children with organic impairment as contrasted with those children whose retardation appeared to be associated with undifferentiated or familial etiologies; the greater the changes made in the environment, the greater were changes in rates of growth, e.g., children removed from inadequate homes and put in foster homes while attending the experimental school increased their rate of development, and children living in culturally deprived homes who did not attend the preschool either remained at the same rate of development or dropped to a lower level. During the preschool period, the Community and Institution Experimental Groups increased about ten points in IQ and SQ (Social Quotient) and more or less maintained their gains after this period; the Community Contrast Group retained their original scores during the preschool period and, by the time they had attended first grade or special class for a year, their IQs and SQs began to approach the scores of the experimental children; the Institution Contrast Group had a drop in scores during the preschool period without acceleration in rate of growth after they attended the institution school. Kirk concluded the following from his equivocal data:

> It would appear that, although the upper limits of development for an individual are genetically or organically determined, the functional level or rate of development may be accelerated or depressed within the limits set by the organism. Somato-psychological factors and the cultural milieu (including schooling) are capable of influencing the functional level within these limits. This theoretical position appears to be the most tenable in the light of the evidence herein presented (p. 213).

Kirk's study represented a marked advance in research on mental retardation. For one thing it attempted to combine an empirical and experimental approach with the clinical case-study approach, thereby reflecting the complexity of the relationships among variables within individuals and between groups at the same time that it illustrated the difficulties of research in this area. It should also be noted that an attempt was made to control for examiner bias, although it is not clear from the too brief statement on this point how successful this attempt was (1958, p. 16). What is especially clear in Kirk's study, and sets a high standard for future research, is the careful description of the process and problems of subject selection, an aspect generally handled superficially in most other studies in this area.

A limitation in Kirk's study is the small number of cases in each of his groups, the reasons for which are made clear in his discussion of case selection. Further difficulties of case selection made impossible the random assignment of cases to the different groups. It should also be mentioned that the educational environments to which the different groups were exposed are not described in the detail necessary to guide efforts of replication by others or to allow one to determine what aspects of the school environment may have contributed to the findings reported by Kirk.

Fouracre, Connor, & Goldberg's more recent (1962) five-year longitudinal study on the effects of nursery-kindergarten experiences on the immediate and subsequent behavior and adjustment of preschool educable retarded children disclosed problems and findings somewhat similar to those reported in the aforementioned study by Kirk. They had even greater difficulty in locating preschool mentally retarded children between the ages of four and six, especially those without central nervous system impairment. In fact, this difficulty proved to be such a major handicap that it required broad revisions and departures from the original plan of the study and reduced its relevance to our purposes.

Two recent studies used samples of children, procedures, and tests very similar to those reported herein. Gray & Klaus (1965) followed a sample of 60 children for three years in a medium sized southern city. They found that, prior to school entrance, their experimental groups who received a special preschool program showed significant gains when compared to a control group. Alpern (1965) failed to find differences between two groups of children (N=30) who had and had not been exposed to a one-year nursery school enrichment program. The original mean IQs of Gray & Klaus' sample was 87 as compared to 94 for Alpern's, the latter sample being quite similar to the present sample.

Conclusions

In this chapter we have attempted to discuss certain research studies and points of view considered by us to be most relevant to our own research problem:

the effects of a variety of preschool experiences on the performances of children coming from backgrounds, or geographical foci in our community, known to contribute disproportionately to the mentally retarded population. This selective review seems to permit the following conclusions:

1. At the present time there appears to be a marked resurgence of interest in mental retardation generally and in the cultural-familial type of case in particular. Whereas in earlier decades the cultural-familial cases (variously labelled "Kallikak," garden-variety, subcultural) were viewed as a distinct etiological grouping of genetic origin, they tend today to be viewed as part of that much larger problem group in our society given the label "culturally deprived."

2. There seems to be general agreement that genetic processes represent an important source of influence on the biological foundations of intelligence. There also seems to be an increasing recognition that far too little is known about the nature of intelligence (except, perhaps, that it is vastly more complex than is indicated by the usual IQ score) to justify drawing anything resembling specific hypotheses about the role played by genetic factors. Put in another way: the heated nature-nurture controversies of the past have been superseded by the recognition that earlier formulations were oversimplifications which served the participants' personal opinions far better than it did clarification of the problem.

3. The above change in viewing the nature-nurture controversy, together with the emergence of cultural deprivation as a major problem in our society, seemed to set the stage for systematic attack, both in research and in social action, on ways of bringing about environmental changes that might prevent intellectual deficits. Put more positively, the aim seemed to be to intrude

into and to change environments in order to determine the degree to which intelligence in these individuals could be educated, i.e., to evaluate what one "could bring out" under changed conditions.

4. There are relatively few systematic studies which bear directly on the effects of planned interventions on the intellectual development of culturally deprived or cultural-familial mentally retarded children. The studies which have been done vary greatly in methodological sophistication, quality and quantity of descriptive detail about such important variables as selection of cases, differences in contrasting environments, and control of bias in collection of data. The findings tend to support the conclusion that planned interventions have the predicted effect of increasing intelligence test scores, although it is by no means clear what aspects of the environment are the most important ones. Perhaps the most cautious conclusion one should draw is that available studies do not allow one to conclude that the problem is solved.

5. It may well be that one of the major difficulties encountered by recent studies may in itself turn out to be one of the most illuminating aspects of the development of children from culturally deprived or cultural-familial backgrounds. We refer here to the fact that although they can be found in great numbers in the school setting, the mildly mentally retarded children of preschool age without central nervous system defect were extremely difficult to locate even when special efforts of case finding were made in neighborhoods where one would expect to find them in fair number. One possibility, of course, is that intelligence tests measure different abilities or behaviors in the preschool period than in the school years. However, there is no evidence that this possibility could account for more than a part of the difficulty in case finding. Another possibility assumes that, in as yet

undetermined ways, introducing these children into the school culture maximizes a conflict between the home and school cultures producing attitudes toward learning and self that negatively affect test performance. In any event, if the difficulty in case finding is a real one, the explanation of it becomes of major significance in future theorizing and research.

It is probably not necessary to elaborate on the final conclusion that research involving planned interventions in the life of young children is unusually complex--in terms of conception, execution, and interpretation--and recognition of this complexity should influence one's perspective of the work of earlier investigators and moderate one's expectations about the speed with which answers will be forthcoming in the future.

Chapter III

Subject Identification and Selection

The focal strategy of studying mental retardation by selecting and treating essentially normal preschool lower-class children was a direct product of our theoretical position regarding the relationships between social class and intellectual development. Changes in selection criteria were a result of many months of casework and consequent deliberations. Since these changes were closely connected with design modifications, they have implications for research methodology. But this kind of field inquiry does not lend itself to clear distinctions between methods and results. The way a problem is studied is theoretically significant for both particular results obtained and the way behavior is viewed by investigators. Therefore, considerable attention will be given to our process of changing original criteria.

It will be remembered that, despite the high incidence of mild mental retardation without organic pathology in lower-class communities (Fouracre, et al., 1961; Kennedy, et al., 1961; New York State Department of Mental Hygiene, 1955), there has been great difficulty in locating such cases at preschool age. This finding has led to two major hypotheses: (a) these preschoolers cannot be identified because of diagnostic naiveté and inadequate tests; (b) identification is difficult and cases are infrequent because the retardation of school-age children is a function of an interaction between elementary and preschool experiences rather than of abilities and inferred capacities of preschool children. Without rejecting the first alternative there is a fair amount of evidence that the second is an hypothesis that deserves careful consideration. This study is mainly concerned with this second hypothesis. Suffice it to say at this point, the children were selected from lower social class environments and they were at least two years away from entering first grade.

The sample obtained had a relatively small incidence of children who could be psychometrically designated as being mentally retarded. From knowledge about the community under study and from that which is assumed to be generally true for lower-class school-age children, it was expected that, in subsequent years, this sample would have a considerably greater incidence of mental retardation.

Original Criteria

Originally, it was the intent of this project to select preschool siblings of cultural-familial retarded school-age children in order to determine whether specially devised preschool experiences would significantly effect their academic ability and estimated potential. From the outset, a diagnosis of mental retardation for these preschool children was not a criterion for selection. This procedure reflected our position that case finding difficulties of previous investigators were substantive rather than methodological in origin, a position congruent with our own experiences in a variety of educational and clinical settings. In addition, we felt it reasonable to assume that if one waited until such a diagnosis were possible, it would then become more difficult, and perhaps impossible, to reverse the retardation. We felt it equally reasonable to assume that if a familial group of older retarded siblings were selected as a reference group, the younger siblings could be expected to develop in somewhat similar patterns without outside intervention. Therefore, subjects were originally to be selected from a population of preschool children where there was a strong likelihood of mild mental retardation--without accompanying central nervous system involvement--within the families of these children. Further, in order to maximize the likelihood that our preschool population could be expected (without intervention) eventually to function at a mildly mentally retarded level, the original criteria stipulated that subjects selected would have both a mentally retarded older sibling and a mentally retarded parent, each without any organic involvement. In summary, the original criteria were that subjects: (a) come from a lower social class, (b) be of preschool age, (c) have at least one older retarded sibling, and (d) have at least one retarded parent. These criteria are very similar to those for cultural-familial retardation listed in the American Association on Mental Deficiency Manual on Terminology and Classification in Mental Retardation (1959).

Possible alternative procedures for case finding follow directly from the criteria. From lower social class environments, one might obtain lists of: (a) preschool children, or (b) mentally retarded school-aged children, or (c) retarded adults. The first alternative was impractical and, besides, no such list was available to us. The third alternative was eliminated as a possibility because, as was pointed out in previous chapters, adults having a history of mild retardation usually become assimilated into the population and often cannot be identified as retarded. Therefore, in our initial thinking, the second alternative was selected as practical and sound: lists of mildly mentally retarded children in the public schools of a lower social class area would be obtained and, if there were a preschool sibling, the parents would be interviewed in order to ascertain their intellectual functioning and to secure their cooperation for including their child in the study.

To obtain a small pilot population with which to work, cooperation was secured from the cities of Waltham and Newton, Massachusetts, during the fall and winter of 1961. Their school departments agreed to provide the project staff with lists of special class children residing in lower-class neighborhoods. In addition, the schools contacted each of the families meeting initial eligibility to seek their cooperation and to encourage them to enroll their preschool children in the project. From these lists secured from school officials, 17 children were selected as supposedly meeting the following criteria: preschool age, older brothers or sisters who were mentally retarded and in special classes, mothers and/or fathers who had been either in special classes or school failures, no evidence of central nervous system damage as measured by standard clinical neurological examination, and agreement of families to permit preschool children to participate in the study, either as experimental or non-experimental subjects.[1]

It soon became apparent that this method of selection was unworkable. In these first 17 cases reviewed it was found that, in general, children in special classes either do not have retarded parents or that it is impossible to say anything--without a great deal of equivocation--about the level of intellectual functioning of their parents. In this population, whether or not there is a relationship between retardation in parents and children is a moot point, although our present experiences suggest that, if it exists at all, such a relationship is a complex one. In addition, the determination of the intellectual level of the parents without direct testing (which would cause other problems) is clearly a vulnerable point. It was felt that even an extensive and costly search of school records would not give us this required information. It should be added that school records were not always readily available and, when they were available, did not give meaningful and reliable information.

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[1] The sample of 17 was reduced to 14. One child was dropped because of subsequent diagnosis of central nervous system pathology and the parents of two other children refused to cooperate during the evaluation and placement phases.

To illustrate our difficulty in subject selection, utilizing our original criteria, it may be helpful to present the following case summary of a child from the Waltham-Newton sample considered to come closest in that group to meeting those original criteria:

> Subject 1 and his family have resided at their present address for 11 years. The house is in an alley off a main street in Newtonville. All the homes on this street are in extremely poor condition and are adjacent to lumber yards and other business establishments. The house is in need of repairs, both inside and outside. The rooms are small, dark and dingy. Plaster is torn away from the wall, in many places leaving only beams showing. The furniture, which is sparse, is in poor condition--being torn and broken. The home is littered by debris, including empty beer bottles lying under the bed and garbage on the floor. There seems to be little attempt on the part of the mother to keep the house clean and in order. During visits to the home, the interviewers noted that the children were dressed with torn and dirty clothing and were in need of baths. It was also noted that all of the children were well behaved and friendly.
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> The father of Subject 1 is 36 years of age and went as far as the third grade in school. He then attended vocational school for "a few years." He is presently employed as a laborer, works hard all day and "has not the time to spend with the children that he would like." Mother is 35 years of age and stated that she went as far as the eighth grade in school. There are six children in the family--the two oldest from a previous marriage by the mother. The aforementioned two children are said to attend regular grades. An eight-year-old daughter is in a special class and a seven-year-old daughter is in the first grade, on the waiting list to attend special class. There are two preschool children, Subject 1--chronologically two and a half--is one of them. Mother reports that Subject 1 started to talk at about one year of age, although his speech is still somewhat limited and, in some instances, unintelligible. He started to walk at about eleven months and was toilet-trained prior to the age of two. He gets along very well both within the family circle and with neighborhood children. Although shy at first meeting, he warms up rather quickly and has an active interest in both adults and children.

With the evidence available, can this family be designated as cultural-familial mentally retarded? In one sense, it can. We have here instances of multiple school failures of parents and children. There appeared to be a low level of intellectual functioning of parents. In fact, the project social worker exhibited great skepticism as to whether the mother completed the eighth grade. The social worker felt the father was equally retarded. In addition to the aforementioned description of family life, we had evidence that this family was known to 16 social agencies in the Greater Boston Area, including: Public Welfare, Catholic Welfare, Family Service, Society for the Prevention of Cruelty to Children, and Aid to Dependent Children. There did not appear to be evidences of central nervous system pathology among either siblings or parents to account for the low level of school functioning or community dependency. Notwithstanding these data suggestive of a designation of cultural-familial retardation, we had difficulty in classifying this--our most "familial-like"--family in this category for the following reasons: the reported school successes of the two oldest siblings, the unverifiable school records of the parents, and our incomprehensibility in dealing with the meanings of such terms as "attended vocational school for a few years" and "completed eighth grade."

There were numerous other families in the Newton-Waltham group that presented themselves with more puzzling and ambiguous backgrounds. There were families having a child or two in a special class and other children doing adequate or superior work. There was another family with children in special classes and one parent who had attended special class; however, the other parent attended college for a period of time and one of the children was currently doing well in school. We were not sure what it meant to "attend a Southern Negro college for one year."

We were advised by colleagues well acquainted with the Greater Boston Area that attending (in fact, graduating from) vocational high school does not preclude the possibility of mental retardation. We were further advised that not all children who attend special classes are mentally retarded nor do all who are mentally retarded attend special classes. Although in our dealings with the Newton and Waltham Public School personnel we received an unusual degree of cooperation and were permitted to study case histories of children and their families, we found it impossible to verify all of the data given to us by parents about their educational background, and we found it extremely difficult to understand clearly the intellectual functioning of their children presently enrolled in public schools.

There were several suspicions entertained by the project staff about our inability to locate unequivocal cultural-familial mental retardates. The most obvious and frequently discussed explanation of this phenomenon was that we had just not found a neighborhood sufficiently deprived to offer the candidates we were seeking. Although all of our children selected from Newton-Waltham were clearly categorized in the lower class (using the Warner Index of Status Characteristics, 1960) and although there were sufficient references to the limited school attainments of parents and siblings, it was felt that, in view of the general middle-class character of both Newton and Waltham, it would be possible to find a more suitable geographic area to locate eligible subjects. It should be stated, at this point, that it was clear that the aforementioned families represented both characteristic lower-class socio-economic status and extraordinarily high incidences of school failure. This was manifested and verified in the Sweetser (1962) study of the social ecology of Metropolitan Boston. However, although we were content with our assignment of these families as certainly lower class and, possibly, representative of the cultural-familial mentally retarded, we felt it incumbent upon ourselves to find areas more traditionally and unequivocally designated as severely culturally deprived.

For several reasons, a section of approximately one square mile in Cambridge, Massachusetts, was finally selected for study. First, it was found by Sweetser to be one of the most socially and economically deprived areas in Metropolitan Boston. This area had a high percentage of non-white population, working mothers, residential instability, crowded housing, low family income, male unemployment, low occupational status, and inferior educational opportunities. For several generations it had been the highest "delinquency area" of Cambridge. Two federally supported low-income housing projects formed the central core of this community and the majority of families eventually selected for the study resided in these projects. Other families lived in tenement houses in varying degrees of disrepair, some in better condition than project apartments and others in poorer condition. The vast majority of families in the area were marginally economically independent or were on Aid to Dependent Children or other public welfare.

The community was served by a Roman Catholic elementary and high school which had no special classes and, traditionally, exempted and excluded children who were school failures. There was also an elementary school which had four special classes. The "reputed" mean IQ of this school was about ninety. A few families on the western periphery of the community were served by a second public elementary school.

About 30 per cent of the families were Negro and 70 per cent were white. From evidence supplied by our case finders, it appeared that the majority of white families were Roman Catholic and most of the Negro families were Protestant. Several social agencies served the community. One of the two settlement houses, the Cambridge Neighborhood House, was used as the base of operations for the case finding team. Case work services were provided by the local family casework agency, Catholic Charities, and a case worker from one of the settlement houses. Social workers reported that it was usually necessary for them to visit the homes since mothers found it very difficult to keep appointments. The local child guidance clinic gave some services to these families. Medical services were generally provided by the local city hospital and services for the retarded were extended by the Cambridge Developmental Clinic.

Although the case finding team was able to identify and enlist the cooperation of numerous families that, from any consideration, must be classified as severely socially and economically deprived, we continued to face ambiguous and puzzling evidence bearing on the unequivocal designation of families as cultural-familial. Case summaries of several of our Cambridge families illustrate the problems we encountered:

> The family of Subject 2 lives in a section of Cambridge known as "red block." This block consists of four, four-story apartment houses, all connected. Family 2 lives in a building that was condemned several years ago but never demolished. Many families go to live on "red block" when they are evicted from the project or as a last resort. In this neighborhood, it is considered degrading to have to live on this block. The particular building under discussion is in a deplorable physical condition, dirty, and an apparent fire trap. Stairways are broken and garbage strewn on all floors. Stairways and hallways are dark, with light coming through a skylight during the day. Obscene words are written on the walls of the hallways; the entire house smells of kerosene which is the only type of fuel used; ceilings are cracked and plaster is falling down. The house is infested with rats and this seems to be a continuous problem tenants are faced with. No door has a name or number on it and mailboxes do not give indication as to which apartment contains which family. Most people in the house pick up their mail at the post office, as most mail is in the form of relief or other dependency checks, and it does not appear to be a good idea to rely upon the broken and easily stolen from central mailboxes. It was pointed out that this obscurity helps in avoiding creditors as well as other unwanted visitors.
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> The family is known to 11 social agencies in the Greater Boston Area, including Public Welfare, Catholic Charities, and Family Service.
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> The father is 40 years old, reports having completed seven grades of school, not working, and presently a patient at the Veterans Administration Hospital, suffering with Asthma. Previous to his hospital confinement, he was an odd-job worker. He is said to be an alcoholic.
>
> The mother is 37 years old, reported that she stayed back a lot in school and did not like school but completed seven grades.

There are eight children in this family, six of school age, none in the special class. However, the 14-year-old son is in the fifth grade; the 12-year-old daughter is in the sixth grade; the 11-year-old daughter is in the third grade; the eight-year-old daughter is in the first grade; the seven-year-old daughter is in kindergarten; and the six-year-old son is in kindergarten. There is evidence here of general and multiple grade repetition of siblings.

Subject 2, a three and a half-year-old boy, one of two preschoolers in the family, was delivered after a normal pregnancy. The mother reports an uneventful early life, he ate well, was weaned without difficulty, walked at about a year and talked at about a year. His toilet-training began at about six months of age and by one year he was trained.

* * *

Family 3 lives in a duplex four-room apartment in Cambridge. The interior of the apartment is in dire need of repairs, very dark, and poorly furnished. During the winter months it is poorly heated and very cold. In general the apartment is very dirty with heaps of garbage on the floors of each room. The toilet and kitchen are unhygienic and neglected. Mattresses on the floor serve as beds and, in summary, it would be difficult to imagine more depressing physical surroundings.

The father is 42 years old and reportedly a graduate of Technical High School. He is a veteran and has always been employed as a welder. He is reputed to be a heavy drinker and to consider his own needs and desires above those of his family. His wife reports that he has not shown any interest in caring for his children or his wife for several years. He has separated many times from his wife, legally during the past year.

The mother is 36 years old and attended school as far as the eighth grade. She reports never to have repeated any grades and considers herself bright, in fact much brighter than her children whom she refers to as "stupid." She has always assumed full responsibility for her family and presently works nights to add to the 30 dollars a week that her husband is required to pay for support of the family.

The oldest sibling, 15 years of age, in good health, just completed the eighth grade at school. In the past, he repeated grades 3, 4, and 6. He dislikes school and is a poor student. The 14-year-old daughter is in good health and considered to be "smartest" of all the children. She began at a parochial school but was removed for school failure. She now attends public school and has repeated the eighth grade. She assumes major responsibility for the care of Subject 3. The 12-year-old son is in good health and considered by the mother to be "lazy." He attends the sixth grade and has repeated the fourth grade. He dislikes school very much. The second youngest child is five years of age and began school in the fall. He is in good health.

Subject 3 was three years old when he entered the project. Although he was an RH-negative baby, he received a clean bill of health during a very closely supervised nine month post-natal period. He has never been hospitalized nor has he had any childhood illnesses. He walked at about eleven months, was able to understand words at about two years of age, and is just now beginning to speak. He completed toilet-training quite recently, gets along very well with peers and family, is considered friendly and easily manageable by the family, and is considered to be brighter than other children in the family.

* * *

The family of Subject 4 lives in a six-room apartment on the second and third floors above a dental equipment firm. Both the interior and exterior of the building are in need of extensive repair. The inside is furnished with only the barest essentials. It is very dirty, windows are broken, and most of the walls are broken away.

The family is known to ten social agencies in the Greater Boston Area, including Public Welfare, Society for the Prevention of Cruelty to Children, State Division of Child Guardianship, Family Society, and Legal Aid Society.

The father is 36 years old, attended but did not graduate from vocational school, and had repeated several grades. He is rarely at home, is said to be of little help to the mother, and spends whatever money is available on "drinking or running about."

The mother is 28 years old, completed the ninth grade of junior high school, while repeating two grades. She quit school at 16 to go to work, married at that time, with frequent separations terminating in divorce in 1959. Since her divorce, she has been receiving Aid to Dependent Children assistance. She appears unable to keep up with rearing her seven children. They are physically unclean and unmanageable.

The oldest sibling is ten years old, recently completed the fourth grade, and has repeated the first grade. The nine-year-old daughter has just completed the third grade and has not repeated any grades. The seven-year-old daughter has repeated the first grade. The six-year-old son has just completed the first grade and is going to repeat it this year. The five-year-old son completed kindergarten this year but will repeat it again next year.

Subject 4, three and half years old on entrance to the project, is one of two preschool children in the family. His early history was normal and unremarkable. He reportedly said words at about one year of age, walked at 11 months, and has been toilet-trained since his second birthday. He gets along well with other children in the neighborhood although he does not get along with his brother, one year older than he. His mother reports him as being "spoiled." He is the baby in the family and apparently both mother and siblings "spoil him."

* * *

The family of Subject 5 lives in a five-room project apartment. The interior of the apartment is neat and clean, although sparsely and poorly furnished. There is some attempt to keep the apartment in good order.

The family is known to nine social agencies, including Public Welfare, Family Service, State Division of Child Guardianship, and the Society for the Prevention of Cruelty to Children.

The father is 38 years old, completed six grades of school and repeated at least three grades prior to his leaving school at age 16. He has always worked as a fisherman. During the fishing season he leaves his family for long periods of time and when he is home he spends his evenings drinking, gambling, and "running around." He is reported to be an ill-tempered person, easily angered and unconcerned with the financial or emotional support of his family.

The mother is 30 years old and attended part of the second year of high school. She left school at age 16 in order to get out of an unhappy home situation, married at that time, and is presently suing for divorce. Since her separation she has been receiving Aid to Dependent Children funds. Because her husband is frequently away from home, child rearing is left almost entirely to her. She feels she is too easy on the children, not strict enough and, as a result, the children get what they want.

The oldest sibling, a daughter, has just completed the eighth grade and has never repeated any grades. A son, age nine, repeated the first grade and is now attending special class. He is a "fire setter" who was sent by the courts to a child guidance clinic and is presently awaiting treatment. A son, age eight, has completed the second grade and has not repeated any grades. A daughter, age five, just completed kindergarten and is going into the first grade.

Subject 5, one of two preschool children in the family had an uneventful early childhood, talking about the same age as the other children, and walking by the time he was one year old. He was toilet-trained by the time he was two and one-half, although he still has accidents at night. He is a pleasant little boy, minds his mother well, responds to her discipline, rarely has to be spanked, is good natured, and mixes well with other children in the neighborhood and his siblings.

* * *

The family of Subject 6 lives in a wooden house outside the project area. The house has broken windows, broken wallboards, paint cracked and peeling, and garbage, glass, and other debris strewn around the yard. Torn shades and broken windows can be seen from the outside of the house. The front entrance reveals a garbage cluttered portal and narrow dark steps, unlighted and smelling of gasoline. Inside, rooms are extremely small, furnishings are bare and in disrepair, and at various places there are barrels of garbage, old rags, and other debris. Beds are not made, four in some rooms, some without blankets, others with clothes or other articles piled upon them.

The father is 38 years old, said by the mother to be a graduate of a technical high school, and is currently working as an unskilled laborer at an automobile agency. He recently was released from jail where he spent one week for contempt of court for failure to pay a bill.

The mother is 40 years old, completed eight grades of school and went a few weeks to trade high school. At the time of her initial interview, she was in her seventh month of pregnancy, expecting her sixth child by Caesarean section. She appears to be a rambling, tangential woman who either has a good sense of humor or extremely inappropriate affect. She appears to have difficulty caring for her children and her stated motive for entering her child in the research project is "to get him off my back for three or four hours a day."

The oldest sibling is 12 years old, mentally retarded, and excluded from public school for a period of five years. He is presently in a special class. The ten-year-old daughter is in the second grade. The nine-year-old daughter is presently repeating the first grade and the six-year-old daughter is repeating kindergarten.

Subject 6 was born by Caesarean section, exhibiting a slow developmental history. Although he walked at an average age, at age three and a half when he entered the project class, he was just beginning to talk. He eats poorly and had been hospitalized where a diagnosis of anemia on the basis of malnutrition was made.

* * *

The family of Subject 7 lives in one of the two aforementioned housing projects. The apartment is dirty, barren of furniture, extremely crowded (although this is a five-room apartment), and, in general, quite dilapidated.

The family is known to 11 social agencies in the Greater Boston Area, including Public Welfare, Family Society, and Legal Aid Society.

The father, age unknown, is rarely home and the mother has little idea what his educational attainment was. Mother describes him as "drunk all the time and there's no point interviewing him."

The mother is 39 years old, toothless, and just returned from the hospital where she gave birth to her eighth child. She completed three years of high school.

The oldest sibling, 18 years of age, is a special class graduate, went one year to vocational school and is now "away." A 17-year-old son is in the first year of trade school. A 13-year-old daughter is in the special class. A nine-year-old son is in the first grade. A six-year-old daughter is in kindergarten.

Subject 7, one of three preschool children in the family, was approximately four years of age on entrance to the project class. He appears to be an appealing child, inhibited and largely non-verbal. He is of average size and does not have any noticeable physical disorders.

Obviously, the above families, not unrepresentative of our entire study population, can be categorized as culturally deprived and exhibiting high incidences of school failure, both of parents and children. However, as with our experiences in Newton and Waltham, we were unable to verify the intellectual level of parents nor were we always able to understand the school attainments of children presently enrolled as students. In the Newton-Waltham sample, our problems were due mainly to difficulty in ascertaining intellectual levels of parents without the obvious embarrassments and complications that would accrue in attempting to evaluate them directly. In addition, we had some difficulty in understanding both the school records of children presently enrolled and the inconsistent school records within families.

In the Cambridge population we were beset with an additional problem: because of stringent school regulations we were unable to secure permission to study case records of children who were either presently attending or had completed formal schooling. (This problem was mitigated later, as we developed a better relationship with the administration.) However, from our experiences in Newton and Waltham, we had to conclude that even had school records been available in Cambridge, they would not have given the type of information which we regard as both meaningful and reliable. In addition, we were forced to conclude that, although all of our families could be classified as culturally deprived and although there were inordinate amounts of school failure and inattention to intellectual stimulation in these homes, and notwithstanding the fact that some of our families could be classified as "cultural-familial mental retardates" using the aforementioned American Association on Mental Deficiency nomenclature, it appeared from our studies of cases that the occurrence of documented mental retardation in the parent was relatively independent of its occurrence in the child. It was, therefore, not possible to obtain a clear, unambiguous sample of any size if the A.A.M.D. criteria for cultural-familial mental retardation were to be met in letter as well as in spirit.

Final Criteria for Subject Selection

The final criteria adapted for subject selection did not include either having siblings in a special class or having a retarded parent, although, as it turned out, many subjects met these criteria. The most important criterion employed was that the subjects had to reside in a highly deprived area characterized by high delinquency rates, a considerable proportion of school drop-outs and school failures, low occupational status of parents, and run-down homes. The other criteria employed were: no evidence that the family was living temporarily in the area, the level of parental education and occupation was usual for that area, and neurological examination of the child revealed no central nervous system pathology. Finally, the parents had to give their consent for inclusion of the child in the project.

As mentioned previously, the problem of obtaining names of families having children age-eligible for the project at the time of subject selection was not facilitated by access to school records, as in Newton-Waltham. Nor were records of the housing projects available. The first step in obtaining lists of possible candidates involved discussion with executives and workers at community agencies who might have knowledge of families in the area meeting our criteria. Representatives of Catholic Charities, Cambridge Family Society, Cambridge Neighborhood House, Margaret Fuller House, Roberts School, St. Mary's School, Visiting Nurses Association, Department of Public Welfare, Cambridge Developmental Clinic, the neighborhood Catholic church, and the housing projects were visited and the study discussed with them. Volunteer workers at the Cambridge Neighborhood

House, where the case finding team was based, met with the project staff to discuss our need to locate preschool children. Fifty volunteers--college students from Boston University, Harvard, Radcliffe, Lesley, Simmons, and Wellesley--agreed to canvass both of the housing projects and the apartments on the surrounding streets. They returned with lists of families who had children between the ages of two and a half and four, at least age-eligible for the project. Those families who were interested in applying for our school were instructed to call the Cambridge Neighborhood House to make an appointment for an interview and evaluation of their child.

Approximately twenty mothers phoned the Neighborhood House for appointments to discuss the school with the project staff. All except one mother kept her appointment at the stated time. For the most part, the people who called for appointments were not eligible on the revised criteria, i.e., they were not characteristically from lower classes, they were temporarily in the community due to one setback or another, or the preschool children were not eligible for inclusion in the study due to a physical or psychological problem.

It soon became apparent that the project staff could not set up office in the Cambridge Neighborhood House waiting for parents to come in to be interviewed for the project. Therefore, unscheduled home visits were made to all families who had given their names to the volunteers as having children age-eligible for the project. Staff members had little difficulty in obtaining entrance to these apartments when they announced that they represented the Cambridge Neighborhood House and, consequently, families were visited in this manner. Some of these families did not meet one or another of the criteria for inclusion. In 12 cases, most of whom were eminently eligible for the project, mothers declined to participate because they wanted to keep their preschool children at home. Two fathers indicated that they did not wish their young child to go to nursery school because the others had not gone and they would not wish this particular child to get ahead of his brothers and sisters.

Several of the families who were accepted via the home visitation method subsequently failed to keep appointments at the Cambridge Neighborhood House and all interviews and psychological and physical evaluations had to be conducted in the home setting. It was predicted by the case finding staff that these, and numerous other families, would have difficulty getting their children ready for school and bus pickup, were they to be accepted as experimental subjects. Such predictions or expectations were not involved in final selection. If the family met the criteria and agreed to have their child participate, the child was included.

In addition to the two aforementioned methods of case finding (scheduled and unscheduled home visits), there still remained a sizeable minority of families who were "hard to reach." They had not been contacted by the volunteers nor had they responded to flyers sent out to every home in the neighborhood suggesting that they call the Cambridge Neighborhood House for information about the preschool classes. To contact this group, a period of "cold canvassing" was instituted by the project team. The method employed was a simple one. A staff member walked along a street and if he saw a particularly dilapidated house in which it appeared no one could possibly live, he knocked on the door and usually turned up a family which conformed admirably to the criteria. In addition to the families obtained by home visits, interviews within the Neighborhood House, and cold canvassing, several candidate-families were suggested by the staff of the social or educational agencies previously mentioned.

As a result of what we believe to have been a thorough search for eligible children, employing a variety of reasonable methods to find subject-candidates, 69 subjects from the Cambridge area were found to meet the criteria. Five were ultimately dropped because mothers never sent them to the preschool program, two experimental children moved out of the area, and two non-experimental children moved out of state--thus reducing the sample to 60 children. Together with the pilot sample, there was a total of 74 cases meeting all criteria and accepted as part of the project.

Implications of Case Finding

It is obvious that the criteria finally employed do not allow us, directly, to generalize our findings or conclusions to the cultural-familial mentally retarded group, as that group has traditionally been defined. This restriction may be far less important than it seems in light of our experience, similar to that of other investigators discussed in the previous chapter, that the preschool cultural-familial child is difficult to locate. The degree of difficulty which we and others have encountered, when taken together with the relative ease of locating such children during the school years, suggests that an explanation in terms of poor measuring instruments is far from a complete one.

It may be that for reasons now poorly understood, or not even as yet stated, the cultural-familial family exists in far fewer numbers than in earlier decades. This is not to say that there are not certain neighborhoods and, in fact, particular families that breed large numbers of so-called familial mentally retarded children. Nor do we imply that these neighborhoods are decreasing in size. The point we are emphasizing is that it is becoming more apparent that the clear-cut, easily categorized familial family is less and less available for study and more and more difficult to explain. For example, if one were to review some of the earlier family studies presented by Goddard (1912) and other workers, it would have been fairly easy to categorize certain families as familial, based on currently accepted criteria. In those families it was usual for both mother and father to be in special classes or to be early school drop-outs or school failures. It was also quite usual to find several of the children either in special classes, institutional programs, or school failures. Our experiences have disclosed that those families that are now found often present such confusing discrepancies with the stereotype "cultural-familial mental retardation" that it is very difficult to designate them as familial, even though they meet the minimum criteria. When one considers the dramatic changes which have occurred in our society since the early decades of this century, it is by no means far-fetched to assume that they have operated to reduce the number of such families. Acceleration of urbanization of our society, the great advances in transportation and communication, the increase in special education facilities, the ever-increasing number and quality of social agencies--these and other changes conceivably may have had the consequence of reducing the number of cultural-familial families.

Although the nature of our subject population restricts us from generalizing directly to a population of cultural-familial mentally retarded children, it does seem that we can generalize, however cautiously, to a much larger population. It will be remembered that the basic consideration in selecting subjects was that they come from an environment which had a history of producing a high percentage of school failures. This kind of environment has come to be referred to as a culturally deprived environment. There is good reason to believe that such environments exist throughout the United States in cities and in rural areas.

They are characterized by low incomes, high unemployment, high delinquency rates, a great dependency on social welfare agencies, and a high incidence of school failure in the local schools. Not only is there assumed to be a great similarity in the symptomatic social behavior within these neighborhoods but it is also assumed that the deprivation that is operating upon individual children is more or less homogeneous from area to area.

It is, of course, plausible to entertain the question of different kinds of cultural deprivation that exist within different kinds of communities. However, for the purposes of this study, it seems reasonable to assume that, within the variety of circumstances that exist in lower-class environments, there is a substantial core of communality which is more a function of the conditions that exist within the environment than it is a function of the biological characteristics of the children within these environments. Without making any judgment as to how much weight can be given to the environmental characteristics, on one hand, and the biological characteristics, on the other, it is assumed that the weighting of the environmental characteristics is sufficient to make programs such as will be described in this volume generally applicable.

It is clear that there has been no explicit attempt in this study to choose a typical lower-class community or to select typical children from that community. A rigorously representative procedure was impossible to accomplish, both because of the tremendous expense involved and because of the characteristics of lower-class communities. In such communities the researcher cannot carefully explain to all of the parents what research is and how it will be accomplished nor can he assume the environmental constancy that might be expected in a middle-class setting. In spite of the fact that the selection of the community and the children within the community were not done in a way to insure that they would be representative, it was felt that a study of obtainable children in any lower-class community could be extremely meaningful provided that the children could be randomly assigned to experimental and non-experimental groups. The difficulty here centers about whether effects of the experimental procedure, if they prove to be significant, are externally valid even if the effects are true ones. This is to say that any significant differences that are found to be valid for the particular group of children in the study might not be valid if generalized to other groups of children in the same community or in other communities. The question we ask is whether the variability that exists within the sample of study children is any greater than the variability between this sample and other possible samples of children in this community or in other communities.

There is, of course, no way for us to fully resolve the above question at the present time. Follow-up studies of these children in the years to come could certainly determine the extent to which they are more or less typical of the community in which they live. The assumption under which this study has been designed is not so much that these children are typical of lower-class children but that they are not atypical and that their responses to being in a particular kind of preschool environment for two years will not be unlike the responses of other lower-class children because of the great similarities that do exist in the homes and in the general circumstances of marginal families.

This study will have external validity to the extent that there is homogeneity among lower-class communities with regards to the environmental effects upon educational functioning of children that live within them. To the extent that there are biological determinants of behavior that are specific to different kinds of communities, there will be a question about external validity. Dobzhansky (1962) has pointed out that the effect of environment on organisms will vary according to how deviant the environment is. To give an example, an environment which is extremely hostile to an organism will play a much greater part in that organism's development than would a more normal one. One can reason from this that experimental environments will have greatest effects on those children whose home environments have been most extraordinary.

In summary, the selection of subjects for this study had two major dimensions: in the first place, we obtained a sample of children whose probability for school success was by no means high; and, in the second place, this sample seemed to be not unlike other samples of children in other communities that are characterized by a high incidence of school failure.

In Chapters V and VI we shall describe, at length, the preschool environment into which part of our sample was placed. However, before presenting such descriptions, it is necessary to describe how the experimental and non-experimental groups were formed, the selection of psychological and social evaluations employed, and other matters having bearing on the research design and problems of interpretation.

Chapter IV

Design for Group Assignment and Test Selection

In Chapter III we described our rationale for, and our activity in, selecting a sample of lower-class preschool children. Fundamentally, our interest resided in the study of the problematic relationship between cultural deprivation and mental retardation in the context of ongoing educational programs. This led to the operational problem of assigning subjects to groups and the selection of measurements which are specific concerns of this chapter.

While it is a fairly straightforward matter to set up an experimental design and to select measurements in order to evaluate the effects of certain specified treatment programs, it by no means follows that the application of that design in field research can be easily communicated to other investigators. For example, it is perfectly clear to social scientists what is meant by randomized groups, at least at that moment in time when subjects are actually assigned to one of several treatment or control groups. However, it is not at all clear what this random assignment means when there are experimental children who do not receive the full treatment and when there are control children who do not stay "controlled." It may very well be, and it is our contention, that a simple description of principles of experimental design as they apply to a particular study, without a careful discussion of how and if these principles worked, can do more to distort a description of what has taken place than to contribute towards its understanding.

In this light, our view of design is more that of interactions among statistical, logical and measurement principles, on one hand, and application, on the other, than a simple description of a plan that was designed before the intervention started. We will often refer, specifically, to what was planned before the formal phase of the investigation started but we will try to make it clear that the vagaries of field research often caused us to depart from our plans to such an extent that the departure was more significant than the original plan itself. In presenting the design in this way we have in mind to formalize the crucial problems of educational field research, particularly with reference to those studies that concern themselves with lower-class preschool children whose surroundings and opportunities can be loosely described as deprived.

Design of Groups

In Chapter III we described the selection of two samples of children which were to be used to test hypotheses about the effects of an educational intervention during the preschool years. The division of these samples into experimental and non-experimental groups is graphically described in Table 1. The pilot sample, which was described in Chapter III, was organized a year before the principal sample and provided the project staff with an exploratory group. This group permitted us to study selection, testing, and curricula procedures before the more formal phase of the study began. Furthermore, it gave the senior staff of the project the necessary time to train teachers for the classroom and for work with children in a specially designed teaching situation--the Responsive Environment. This method utilized an electric typewriter to enable children to learn through their own discovery.

The division of the principal sample into two experimental groups and a non-experimental group was done by stratified random assignment, utilizing the Stanford-Binet IQ, chronological age, and sex in the stratification. This prior stratification assured maximum efficiency and group equivalence (Deming, 1950).

The designation of the group of children who were not involved in the intervention as "non-experimental" rather than "control" is the result of our insight into the design of field research of this type.

Table 1

Design of Groups

	Analytical Categories		Samples	
	Primary	Secondary	Principal	Pilot
Treatment Groups	Experimental	Preschool and Responsive Environments	N = 18 $\overline{CA}^1 = 3.2$ Males 11 Females 7	N = 7 $\overline{CA} = 5.0$ Males 3 Females 4
		Preschool only	N = 20 $\overline{CA} = 3.2$ Males 11 Females 9	not applicable
	Non-Experimental	Home Environment	N = 21 $\overline{CA} = 3.2$ Males 13 Females 8	N = 7 $\overline{CA} = 5.3$ Males 3 Females 4
Totals			$N = 59^2$	N = 14

1) CA Chronological Age Mean at time of first testing (July 1962).

2) One subject was lost during the fourth testing, original N = 60.

The 21 children who remained home were certainly not a control group in the sense that they received nothing. On the contrary, during the first year of the intervention five of these children were involved in preschool programs in their immediate neighborhoods. Further, during the second year of the intervention 13 of them were involved in a kindergarten program at the local public school--one that consisted of classes which were relatively small (approximately eight to ten), because of the large exodus of children from the neighborhood into our experimental program. In addition to this obvious contamination of the control process there was similar contamination of the experimental process as not all of the experimental children were in daily attendance in the experimental program and there were a few who rarely attended the program in the two years of its existence (See Tables I and II of Appendix A). For these reasons, the apparent discreet dichotomy between experimental and non-experimental is, in reality, a continuous variable which includes children who had highly stimulating interventions for two years, either in or out of the project, and children who received practically no preschool or kindergarten program prior to their entrance in public school and, therefore, prior to the final evaluation.

We see this impurity of the independent variable as an ubiquitous problem in field research of this type, especially when investigators focus their attention on the intensive study of relatively small groups of children.

One strategy for dealing with this problem was to compare experimental groups with each other as well as with a non-experimental group. In the present investigation two experimental groups were formed--one which received the preschool intervention plus the Responsive Environment and a second group which received only the preschool intervention. As a result, there are two analytical categories, or approaches, that may be used in analyzing our data and in looking at the results of the various programs. The primary categories simply divided the sample into an experimental and a non-experimental group. Analyses of data were made on this basis. Secondary categories concerned the random division of the experimental group into the two previously mentioned sub-groups. In both analytical categories, the same non-experimental group was used as a basis of comparison. The implicit problem in the use of the secondary categories concerns the question of whether the three groups should be compared or whether each of the experimental groups should be compared separately with the non-experimental group. In either case it was felt that the use of the secondary categories depended upon establishing the effectiveness of the experimental treatment, including both variations. Once the primary analysis could be accepted as significant, it was thought that a secondary analysis could be made in order to test the relative effectiveness of the variations in the experimental program.

From the above discussion it can be seen that subjects were randomly assigned to groups and, as will be described later, groups were systematically tested at four yearly intervals, all of which points to the structure of a true experimental design. As has been pointed out before, the problems of field research were such as to contaminate the purity of this design.

It should be added here that an important threat to the internal validity of many studies in this area, subject attrition, was not a problem in this investigation. During the three years in which children were involved only one subject was lost (during the last year), although several of the experimental subjects moved out of the area and therefore could not continue in the intervention. Whenever experimental or non-experimental subjects moved, we continued to follow their development with home visits and with testings at the stipulated times. As a result, all subjects, but one, were maintained with respect to their original designation as either experimental or non-experimental.

Not only is it important to underline issues concerned with the evolving design of this study, but also to stress that design includes the decisions that must be made along the way. It is our contention that the issues that are raised in this chapter are not merely mundane problems or questions about what happened after the study was designed. We feel that they should properly be treated in a section dealing with theoretical problems of design, whether or not such problems were worked out prior to the initiation of an investigation.

There are two problems, namely, who tests experimental and non-experimental children and where they are tested, which are important issues in a field investigation, particularly one involving disadvantaged children. We made careful provision for, and went to great expense to obtain, unbiased testings in regard to biases due to prejudices about the general nature of the project as well as about the experimental--non-experimental designation of particular children. During all testings, psychological examiners who had no previous connection with the study and were, therefore, disinterested in its results were employed. Every effort was made to assure ourselves that the psychological examiners were unaware of whether any particular child was an experimental or a non-experimental child. This "blind" was completely successful in the third and fourth testings (see Table 2), partially successful in the second testing, and not applicable in the first testing because, obviously, groups were selected after the testing. Furthermore, great care was taken to have each testing take place in a well-controlled situation for both experimental and non-experimental subjects. To achieve this control, all of the study children were brought into a common setting for a testing period in May of each year following the first year of intervention.

This discussion has been concerned exclusively with those problems of design which were related to the internal validity of this study, i.e., whether the obtained results were a close approximation to the "true" results. Of chief import to the question of internal validity, were the randomized nature of subject assignment and the maintenance of the sample throughout the three-year period of the study. In addition, instrumental threats to the internal validity were carefully attended to: in the way tests were administered, the maintenance of a "blind" in regard to the experimental or non-experimental designation of each subject, and the location and situation in which testings took place.

The external validity has been implicitly considered throughout Chapter III in the discussion of the case finding procedures. We will further attend to this in Chapter VIII where we deal with the relationship of this project to the community and the crucial relationship between the type of community we studied and the results that obtained. At any rate, the design for the selection of subjects did not deal formally with problems of external validity because it was in direct conflict with the strategy of choice as we viewed it. In order to deal adequately and intensively with children, we designed this study to treat a relatively small sample of children from a restricted geographical area rather than, more superficially, to study a larger sample from a more extended geographical area. Both of these factors were in immediate conflict with the requirements of external validity, but we felt that the depth of our investigation would justify this choice. This was, then, the crucial design consideration in the overall selection of subjects and it was reflected in the way subjects were assigned to groups and in the way the intervention was accomplished over the formal two-year invervention period. Furthermore, it had ramifications in terms of the kinds of data that were collected and the way in which they were collected.

Design of Measurements

A study such as this one which involved many adults and children over four years had a complex personality which can be revealed in many ways. The "Design of Groups" section, and its accompanying Table 1, showed the structure for the independent variable which was the vehicle for studying particular kinds of environmental effects. On the other hand, the measurements that are used in a study reflect the results that are sought, as well as the concentrated areas of interest to which treatment efforts are directed. Although it is probably true that one can understand such a study only through an awareness of all its various phases, it is, nevertheless, just as true that this is just the thing that a reader can not be expected to do, separated in time as he is, from the actual development and execution of the experimental program. That aspect of the study which is revealed in Table 2 certainly reflects that the continual and profound evaluation of children in either a detailed or global sense was not the primary purpose of this study. However, it does reveal that a considerable amount of attention and energy was directed towards the careful and systematic evaluation of the children on a handful of well-known and, for the most part, well-standardized tests.

The original design for this study called for the measurements that are listed for the first testing (see Table 2), which took place in the spring of 1962 towards the end of the case finding period. Also, at the outset, plans were made to include appropriate achievement tests towards the end of the second year of the intervention, which terminated the Cooperative Research funding period for this grant. The point to be made here, is that the involved and extensive nature of the testing during the fourth testing period in May 1965, involved design considerations that took place during the course of the investigation,

Table 2
Design of Treatments, Testings and Variables

TESTINGS AND PROJECT ACTIVITY DURING PRECEDING YEAR

Types of Variables	First (I), May 1962 CASE FINDING	Second (II), May 1963 1st Year of Intervention	Third (III), May 1964 2nd Year of Intervention	Fourth (IV), May 1965 All Children in Public or Parochial School
A. **Cognitive:**				
Aptitude, Achievement, Language	BINET PPVT ITPA[1]	BINET PPVT ITPA[1] LEE CLARK RR[2] MUSIC[2] TYPEWR[3] SCHOOL ACH[2]	BINET PPVT ITPA[1] LEE CLARK RR[2] METRO RR TYPEWR[3] SCHOOL ACH[2]	BINET [BINET SLOPE] PPVT [PPVT SLOPE] TEST ACH[4] SCHOOL ACH[2]
B. **Non-Cognitive:**				
Personality, Social	RORSCHACH VINELAND	RORSCHACH TEST TAK SOCIO[2]	RORSCHACH TEST TAK SOCIO[2]	RORSCHACH ANXIETY SCALES TASC GASC LS DS
C. **Environmental:**				
Home, Family	WARNER FAMILY (Home) FAMILY (Education)	ABSENCES[2] (Project)	ABSENCES[2] (Project)	FAMILY[6] [FAM ACH[5]] [SIB ACH[5]] SIB BEH[5] SCHOOL BEH (School Absence)

Table 2 (Continued)

KEY

Abbreviations	Testing	Explanation
ABSENCES	II*,III*,IV	Absences from preschool or school
ANXIETY	IV	Anxiety Scales for Children (Sarason, et al., 1960) (Raw Score)
TASC		Test Anxiety Scale for Children
GASC		General Anxiety Scale for Children
LS		Lie Scale
DS		Defensiveness Scale
BINET	I,II,III,IV	Stanford-Binet, L-M, 1960 (IQ and MA)
BINET SLOPE	IV	Slope derived from IQs over four testings
FAM ACH	IV	Average school grades of all children in family, including study child
FAMILY	I,IV	Family evaluation; quantitative assessment of interview protocols (8 scales)
ITPA	I,II,III	Illinois Test of Psycholinguistic Abilities (Raw Score
LEE CLARK RR	II*,III	Lee Clark Reading Readiness Test (Raw Score)
METRO ACH	IV	Metropolitan Achievement Test, Primary Level
METRO RR	III	Metropolitan Reading Readiness
MURPHY RR	IV	Murphy-Durrell Diagnostic Reading Readiness Test
MUSIC	II*	Gessell-Ilg Norms for Musical Ability
PPVT	I,II,III,IV	Peabody Picture Vocabulary Test (IQ)
PPVT SLOPE		Slope derived from IQs over four testings
RORSCHACH	I,II,III,IV	Rorschach Inkblot Test, Overall rating of differentiation and form level
SCHOOL ACH	II*,III*,IV	School Achievement of study child rated by teacher
SCHOOL BEH	IV	School behavior rated by teacher
SIB ACH		Average of school grades of sibs of study children
SIB BEH		Average of school behavior of sibs of study children
SOCIO	II*,III*	Sociogram score, Sociogram developed by teachers
TEST ACH	IV	Standardized rating on MURPHY and METRO ACH
TEST TAK	II,III	Test taking behavior as assessed by psychological examiner
TYPEWR	II*,III*	Typewriter test (Only taken by subjects in Responsive Environment)
VINELAND	I	Vineland Social Maturity Scale (SQ)
WARNER	I	Warner Index of Status Characteristics

*Experimental Groups only

Footnotes to Table 2

1. First testing ITPA's not used in data analysis because of incomplete protocols.
2. Given only to experimental groups.
3. Given only to subjects in Responsive Environment.
4. First grade subjects took METRO ACH, kindergarten subjects took MURPHY RR. ACH TEST is combined score.
5. Number of cases is 44 for SIB ACH and SIB BEH because of either unavailable sibs or data, and 49 for FAM ACH as this variable is by family rather than by subject.
6. FAMILY consisted of nine separate scales which were averaged to give each family a score.

above and beyond the original design intent. This is by way of saying that Table 2 tells a lot more about field research as we view it, than a more well-ordered table which "smoothes out the curves" of planning and does not truly reflect the problems of conducting a long-term study on a limited budget and without any guarantee of continued funding. It is also an important design consideration that our application for additional support for this project was not approved and, therefore, the fourth testing in May, 1965, was accomplished independently of any formal funding.

The variables used during the first testing in May, 1962, were primarily to obtain information on subjects in order to stratify our randomization of subjects into experimental and non-experimental groups. The secondary purpose was to provide a base line for the sample of children with regard to those variables listed under Section A, "Cognitive" in Table 2 under the first testing period. These three tests--the Stanford-Binet, L-M, 1960 (BINET), the Peabody Picture Vocabulary Test (PPVT), and the Illinois Test of Psycholinguistic Abilities, (ITPA),*--were used as the dependent variables in order to test hypotheses regarding the effect of the preschool intervention. The use of additional measures in later testings, in the cognitive as well as in the non-cognitive and environmental domains, was dictated by our dissatisfaction with the limited coverage of our dependent variables. This dissatisfaction stemmed from our observations of their relationship to the curriculum intervention, as well as our perusal of the data at the end of the first and second years of involvement with children. In line with this, we proceeded to systematically test all children in achievement and test anxiety. In addition, we did extensive studies of each family during the winter and spring of 1965.

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*Abbreviations of tests are listed and explained in Table 2, "Key."

To summarize, Table 2 represents the design of measurements and of testings as they developed, rather than as they were originally planned, covering an unfunded fourth testing which included achievement test measures and the collection of school grades for all children during their first year of school in either the kindergarten or the first grade. Also involved were the individual administration of the anxiety scales to all the children and the aforementioned intensive study of the families of each of the children.

Rationale for Test Selection

Originally, this study was narrowly conceived as centering on an educational intervention. Although some attention was given to so-called non-cognitive behavior, as well as to the functioning of families of the children involved, the focus of interest was directed to school related activities. Measures of whether children were successful or would be potentially successful in school were obtained. Among others, we reviewed the work of Myers & Dingman (1960) on the "Domains of Abilities of Preschool Children" and explored the possibilities of administering a variety of tests to our pilot sample. We discussed many of the tests included in the seven domains outlined by Myers & Dingman and administered some of these tests to selected children, in order to ascertain their relevance for our educational orientation, as well as for curricula plans for the intervention as we saw them at that time.

However, among other things, we were severely restricted by our budget and by our basic orientation as a demonstration project so that we limited our selection to those tests which we thought would be most efficient in getting specific information, both for the purposes of evaluating the success of our intervention and for assisting us in programming for individual children.

As can be seen in Section A of Table 2 under "Cognitive," we administered the BINET and the PPVT four times to each child and the ITPA three times. In addition, we administered a reading readiness test to the experimental group during the second testing, both the Lee Clark Reading Readiness Test (LEE CLARK), and the Metropolitan Reading Readiness Tests (METRO RR), to all subjects during the third testing and either the Murphy-Durrell Diagnostic Reading Readiness Test (MURPHY RR), or the Metropolitan Achievement Test - Primary Level (METRO ACH), (depending upon whether children were, at the time, in the kindergarten or the first grade in their public or parochial school placement). During the second and third testings, measures of school achievement were obtained by having teachers rate each of the experimental children. During the fourth testing, measures of school achievement were obtained by having all of the subjects, both experimental and non-experimental, rated by their kindergarten and first grade public and parochial school teachers.

There were several important considerations in the selection of this modest battery of tests and ratings used to evaluate the educational success of the sample of children. In the first place, we were interested in a single test that would serve as a predictor of academic success. It was fairly clear that the most adequate test for predicting future school success was the BINET. Although we do not consider this property of the BINET as particularly profound, it is certainly a test that has been carefully developed over a period of many years and, consequently, possesses several important properties. Each of its items has been carefully worked on and appropriately revised so that it is suitable to the age level to which it is directed. Also, averaging a great variety of tested abilities into one global score tends to maximize both reliability and stability. Hence, it is not surprising that this test of current functioning has, heretofore, been found to be a good predictor of future functioning. Furthermore, for our purposes, intelligence was operationally defined as being the score on the BINET. The child who scored relatively high would have a relatively high probability of school success and, it follows, the child who had a high probability of school success would be the most intelligent child. In the same way, the success of the intervention would be a function of the degree to which we made more children intelligent, at least in terms of our operational definition of intelligence.

Although the global nature of the BINET had distinct advantages for us in our total conceptualization of the effects of interventions on children in school situations, there were certain disadvantages due to the very generalized nature of the BINET score. Therefore, from the outset, we used two quite specific tests, the PPVT and ITPA, which seem to center on particular intellectual functions having to do with various aspects of language. In addition, during later testings, we used quite specific achievement tests as well as teacher ratings of verbal and quantitative achievement (SCHOOL ACH), and of School Behavior (SCHOOL BEH).

Another important dimension in test selection pertains to measurement that takes place completely within a particular situation, as opposed to measurement which either covers a series of test situations or which involves a rating of behavior that takes place over a long period of time. In order to get at the first aspect of this dimension, the BINET scores for the four testings and the PPVT scores for the four testings were analyzed. Slope scores were computed so that we had a single measure which indicated direction and intensity of four separate test situations for each child. In order to get at the second dimension, teachers rated each of the children according to particular areas of academic activity.

In addition to these measures, there were several specific tests or scales which were given to selected experimental groups. Of particular importance was the typewriter test, which was given to those experimental subjects who were in the Responsive Environment group, and a music score, which was given to all of the experimental children during the second testing. Since the results of these tests were not particularly meaningful, they are not further considered in this monograph.

It would be pretentious to maintain that the selection of tests was based upon any ponderous theoretical design. The rationale involved the straightforward considerations of using fairly well established measures of behaviors that are obviously educationally meaningful. The use of the BINET and reading readiness tests as a basis for predicting school performance is thoroughly in line with current educational practices. While we do not necessarily endorse this practice in this study, we were willing to go along with it in order to provide our evaluation schematic with external validity with respect to the kinds of tests we used. We were not interested in tests which were designed to assess very limited areas of functioning.

We have a special bias here, admittedly debatable, but one, nevertheless, with substantial pragmatic validity. We believe that teachers, for the most part, interact with children in global ways and for good reason. Research on the development of specific tests, methodologies and programs for the identification and promotion of specific domains of abilities--and for the remediation of specific disabilities--has not been particularly fruitful. Until such time as this problem becomes clearer and teachers receive sufficient help in developing molecular approaches to assessment and intervention, they will continue to utilize more general strategies in the conduct of their programs. This, too, was our direction. However, we continued to experiment with the ITPA and other molecular strategies, particularly toward the remediation of identified deficits that were interfering with the cognitive and/or affective development of particular children.

The variety of the tests used, and the nature of their construction, was in line with our thinking about the generalized nature of curriculum development and the great difficulty of gearing a long-term educational program to the teaching of a few specific skills. Our intent, both in our curriculum and in our measurement, was to improve and measure educability as a generalized function rather than to treat fragments of educability and then to measure them. Obviously, the interventions involved very specific activities, just as each of the tests used involved very specific items, but the total rationale for this project called for their global application and interpretation.

All of the major tests used, as listed in Section A of Table 2 under "Cognitive," nave received a considerable amount of discussion in the literature in recent years. However, in spite of the claims made for the PPVT and ITPA in some of the research with which we were acquainted at the time, they were not particularly useful to us in getting at any unique factors which were not readily accounted for by either the BINET or by the achievement tests that were administered during the second, third, and fourth testings. This disappointing finding limited the extent of our overall psychometric evaluation of the subjects. On the other hand, it is not surprising in the light of the expected difficulty in testing preschool children. The data on all of these tests will be presented in Chapter VII, but it is important to add now, the failure of part of our initial testing program to contribute to the overall evaluation caused us to change our strategy and devote increased effort into the collection of achievement test data, particularly during the third and fourth testings. During the fourth testing period, a considerable amount of energy was spent interviewing every teacher of every child and sibling in public and parochial schools, to get ratings of how each of the children performed in verbal and quantitative areas.

The measures described and alluded to above were selected because of their relevance to the study. No attempt was made to exhaustively assess the cognitive abilities of these children. Such an approach would have been extremely inefficient and quite out of line with the principal directions of the study. The four testings provided us with multiple measurements on each child which served to give us a precise idea of how stable performances were over a three-year period. The variety of tests that were selected, provided us with indications of how the groups of children were behaving on a variety of tasks which had relevance for the experimental curriculum as well as for eventual school success.

Non-Cognitive Testing

The tests listed under Section B in Table 2 are of secondary importance to the major purposes of this study and except for the Vineland Social Maturity Scale (VINELAND), were not included in the original design. Rorschach Inkblot Tests (RORSCHACH) were administered during the first testing by one of the staff psychologists in order to pursue a separate longitudinal study of the RORSCHACH responses of lower-class children during the preschool and elementary school years. The staff followed up on these early RORSCHACHS and they have been administered to all of the study children during each of the testings. A generalized score was inferred from the protocols and used, subsequently, in data analyses. This score was determined by having psychologists "blindly" evaluate each protocol according to both the extent of differentiation and maturity of the form displayed. The protocols were scored on a five-point scale ranging from a highly differentiated good form level response to a refusal to respond to any of the inkblots.

The psychologists who tested the children during the second and third testings rated each child according to his test-taking ability and these data were used in order to find out whether there was any distinction between the score that the child received and his behavior while he took the test. This assessment did not turn out to be particularly meaningful and was discontinued for the fourth testing.

Because of our extensive observations of the children in public and parochial schools prior to the fourth testing, we became more and more interested in non-cognitive factors and in particular, in anxiety as it had been operationally studied by Sarason and his colleagues (1960). Therefore, the complete battery of the Children's Anxiety Scales was included in the fourth testing as an explicit attempt to get more insight both into the possible differences that might obtain between the experimental and non-experimental groups as well as to more fully explore the correlates of school success of lower-class children.

The very limited use of tests in the non-cognitive area is a reflection of the general educational purposes of the study as well as our early negative assessment of the possibilities of validly and reliably administering non-cognitive tests to preschool children. Therefore, the limited use of testing in this area was not a reflection of either our disinterest or our denial of its importance. Rather, it was the result of the practical limitations of a necessarily limited testing program as well as the psychometric limitations of administering personality tests of any type to preschool children.

Environmental Measures

During the first testing, all homes of children in the study were visited, described, and rated using the WARNER (1960), in order to obtain a generalized assessment of the kinds of homes and families that we were dealing with. At that time, there was no provision in the design to formally measure environmental factors in the sense that has been described by Wolfe (1965). Wolfe's work, in collaboration with Professor Benjamin Bloom of the University of Chicago, has gone in the direction of quantifying certain aspects of the "achievement" environment and the "intelligence" environment so that variations in school behavior can be more adequately accounted for. Because of our ever increasing interest in the determinants of school success of lower-class children, we became quite interested in the methods used by Wolfe and his colleagues and we utilized some of their ideas, and added them to some of our own, in designing an evaluation of the family environment for the fourth testing. This was developed and designed with the very specific goal in mind of quantifying the various dimensions of family variation and then using this measure in our study of the correlates of school success both with reference to our intervention as well as with reference to the performance of the study children in their respective public and parochial schools.

We found Wolfe's work to be unsuitable for lower-class families and we were, therefore, forced to develop our own strategies to study those aspects of the home environments which we thought might be meaningful for their school behavior. In order to do this, we employed an experienced female social worker in the winter and spring of 1965. Eight scales were developed, all directly relevant to the home preparation of the child for school. These scales were developed by the social worker in conjunction with the project staff as a result of questions that were repeatedly asked about families in our continuing attempt to try to understand how and why they functioned as they did. For this reason, the scales do not represent a carefully structured and logical taxonomy. Rather, they represent the most pressing questions that were suggested by many hundreds of pages of process reports of interviews with each family. These reports were obtained through repeated interviews by the social worker and a male assistant who carried on interviews with all available fathers. Each family was visited a minimum of two times, and more often, three, four, or five times, in order to obtain extensive information about the parents' and children's functioning with reference to the questions raised by the eight scales.

The scales included the following:

1. Parents' Perception of Child's Functioning as a Student
2. Individual Behavior of Study Child as Displayed to the Social Worker on Repeated Visits
3. Marital Relationship
4. Individual Behavior of Mother and Father as Observed by Social Worker and Assistant on Repeated Visits
5. Mother-Child Relationship as Observed
6. Family Solidarity
7. Mother's Attitude Toward an Involvement in Study Child's Education
8. General Impression of Family Adequacy

Thus, it should be clear that our approach to measuring the environment of the home was not to ask or check off whether certain items were or were not present in the home or whether either or both parents held certain specific attitudes. We found that approach to be unworkable with the families we were studying. For this reason, most of Wolfe's materials were quite useless to us as this was his method for quantifying the dimensions of his homes. Our intensive staffings of homes revealed, in a rather short period of time, that we had to take a global approach to the study of these homes and to ask a series of related global questions about mothers, fathers, and their children. In this way, we felt that we came much closer to measuring essential qualities of the homes than we would have had we utilized a more specific and formalized approach. On the other hand, a serious drawback to our methodology is that it is not easily and directly replicable because it was so dependent upon the case worker and her assistant and the project staff, all of whom took part in the extensive staffings of individual families. However, two sets of independent ratings of the family protocols resulted in inter-rater correlations of .92 and .94.

Conclusion

This chapter has been concerned with the specific operations of this study, including the division of a sample of lower-class preschool children into experimental and non-experimental groups, and the various tests and rating scales that were used, why they were used, and when they were used. In a design such as this one, there is an extremely wide latitude of choice open to an investigator for narrowing down specific problems that can be handled in particular ways. The result has been, as discussed in Chapter II, a great variety of studies aimed at the general problem of educability, with little else in common beside the overall aim. The tack of this chapter has been to present our design, not as a static _a priori_ schematic which is systematically and mechanically operated upon throughout the years of the study but, rather, as an evolving series of decisions and compromises between the theoretical considerations and the realities of budgets, personnel, and possibilities for application.

We have been frustrated in attempts to compare results of different studies that have used subjects and tests similar to ours. We seriously wonder whether the realities of field research, with particular attention to the study of preschool lower-class children, are such that design should be treated as an evolving series of decisions and that communication to other investigators as to what was done, and how it was done, should not be limited to a tabulation of results and then, a discussion of their implications. Perhaps the changing strategies that take place during the years of an investigation provide the kinds of knowledge that are needed, in order to assess the experimental process as well as, of course, the results of the investigation.

Chapter V

The Psychological Climate of the Intervention

We are concerned in this chapter,and the next, with the nature of our interventions with the experimental children. A description of such interventions is necessary and crucial, not only to demonstrate that the experimental children were exposed to a sustained experience not formally available to those in the non-experimental group, but also to convey to other researchers the variety of factors and conditions of that experience.

As was pointed out in Chapter II, one of the deficiencies of most previous studies involving educational treatment has been the lack of detail about the intervention. This deficiency is not surprising when one considers that the intervention usually comprised or reflected a complex set of interrelated factors and conditions. Manipulations and interventions in psychological and educational research are usually and deliberately concerned with no more than one or two variables, or with relatively simple social situations. Too often, there is little realization that simplicity of design and execution is a virtue only to the extent that it mirrors the environmental realities involved. For example, it took behavioral scientists a long time to recognize that, in many areas of research, failure to take into account the personality of the experimenter or interviewer could transform a simple experiment into a simple-minded one.

In undertaking this study, we were acutely aware of two major problems which we could hardly attack at the same time that we were actively intervening into lives of the experimental children. The first of these problems may be put in the form of a question: How does one describe the culture of the "school" we organized and developed? To say that the experimentel groups attended a preschool for two years, while the non-experimental children did not, is not particularly revealing.

As we hope to make clear, the preschool was a continually changing setting involving scores of adults varying on a number of dimensions, a variety of different instructional settings, an array of different instructional materials, and a bewildering assortment of social and professional interrelationships. This school, like any other school, was a subculture in that it had its own traditions, dynamics, and purposes which made those within it feel (to an undetermined extent), apart from the larger society. To conceptualize the complexity of the setting and to develop appropriate methodologies for its description were, we very early realized, problems with which we could hardly cope at the same time that we were pursuing problems of organization and intervention.

The second major problem stemmed directly from the first one, i.e., in any strict sense we would not be able to pinpoint those aspects of the intervention (or those aspects of the setting), which were important or crucial in producing differences between the experimental and non-experimental groups.

Our dissatisfaction with descriptions of earlier studies and our acute awareness of the complexity of the setting in which we were involved sensitized us to the responsibility of attempting to make clear the subtle as well as the obvious factors or variables which may have been experienced by the children in the experimental group. The present chapter is devoted largely to a discussion of those aspects of the intervention which convey something of the atmosphere or psychological climate in which the study took place. The next chapter deals primarily with the curriculum. Together, the two chapters describe the intervention experienced by the children in the experimental groups.

Physical Facilities

The laboratory school in which the present investigation was conducted consisted of several specially constructed classrooms, booths, and offices located in the basement of one of the newer buildings of the Walter E. Fernald State School. This school is a state institution for the mentally retarded located in Waverley, Massachusetts, a part of the Metropolitan Boston Area.

In addition to these structures, one of the regular Fernald School classrooms on the first floor of the building was also made available. This latter room was typical of the attractive, spacious, and well-lighted classrooms that one finds today in modern schools. The room was partitioned into two smaller classrooms. These two, plus two specially constructed rooms in the basement, made four classrooms in all. The basement classrooms were equipped with adjoining observation booths which contained one-way vision glass and were monitored for sound.

Five cubicles, or booths, were constructed so as to be as nearly identical as possible to those used by Moore at his Responsive Environments Laboratory. These booths will be described more fully in the section dealing with the Responsive Environments. The booths were located in the basement, almost directly across a corridor separating them from the classrooms. Lavatories for boys and girls were also situated conveniently close to the classrooms and booths. A special room, where children could be given individual attention as needed, and the project offices, rounded out the facilities within the building. A well-equipped playground, adjacent to the building, was also available to the children.

The choice of the setting for this study was not a difficult one to make, in that it was by far the best of the few available to us. At first blush it might appear very strange, if not self-defeating, to bring the children to a setting that was physically part of an institution for the mentally retarded.

The location, in fact, was no real problem because of the spacious layout of the institution and the fact that the laboratory school was essentially a self-contained unit in a new building at the edge of the institution's land. What did trouble us at the start were the possible negative reactions of the parents to having their children come to such a setting, a problem to be dealt with later in this chapter when we discuss the relation of the parents to the school.

The contrasts between the campus-like institution and the neighborhoods in which the experimental and non-experimental children lived, need not be spelled out in any detail. Suffice it to say that, within and without the walls of the laboratory school, the experimental children were exposed to a setting quite different from that in which they lived.

What perhaps deserves special emphasis is the daily bus trips taken by the experimental children between their home and school. Initially, we regarded negatively the location of the study at the Fernald School precisely because it would involve the expense of transporting the children, as well as raising a host of other problems. It was not until after the study began that we realized there might be some real benefits for the children in being transported. The bus experience was by no means a simple one--psychologically or logistically. It involved the routine of being prepared for the bus (at home and at school), an emphasis on time and the mutuality of relationships and obligations, the need for rules while the bus was in motion, the perception of different neighborhoods while in transit, and the significances of traffic, traffic lights, and street signs. It seems very reasonable to assume that the experience, over time, of being taken from one locale to a contrasting one, might well have been a positive factor.

The Role of Bias

The researcher who is indifferent to the outcome of a study he is doing probably does not exist. The major problem, of course, is to avoid either having the criteria by which one is to evaluate outcomes contaminated by one's bias, or selecting and placing subjects so that the likelihood of securing a particular outcome is unfairly increased or decreased. It was in order to avoid such witting or unwitting effects of bias in our study, that experimental and non-experimental groups were randomly chosen, and different sets of psychological examiners were employed to obtain criteria data. For example, when we became aware that the psychologists doing the second testing were probably aware of the bias of the study(although we made every effort that they not know who was an experimental and a non-experimental subject), we selected psychologists for the third and fourth testings who we were sure did not know what the study was about.

Bias or strong partisan feelings in regard to hypotheses do not have inherently negative effects on research, particularly as in this study, where the intervention involved attitudes toward, and the handling of, children. The writers firmly believed that they could develop a setting which would enable the experimental children to outperform the non-experimental children. We were very much aware that our beliefs had to be communicated through our words and actions to other personnel who would be in daily contact with the children. In diverse ways we were, in effect, saying to the staff (and ourselves): "This is an important problem to study. We have to devise ways to enable these children to be curious about themselves and the world around them, to expose them to a variety of materials, ideas, and contents, and to foster a climate of learning which would give them a solid foundation for normal academic development. If we do this correctly, there is no doubt what the ultimate outcome will be. Forget where these children come from and assume they can do whatever we would like them to do."

It is hard to convey to the reader the degree to which enthusiasm, a spirit of adventure, anxiety, and self-questioning pervaded the relationships among the staff. There were many meetings about many things, and there was rarely a policy question taken up which did not pertain to the effects a particular event, procedure, or interaction had, or might have, on the children. A good part of these discussions concerned individual children, with the focus on why a particular child was or was not doing something, or how he was handled on a particular occasion, or what might be done to adapt the program to his particular needs. One might characterize the behavior of the staff as reflecting the implicit assumption that anything done in relation to the children could be justified, only if it clearly served the purpose of facilitating a type of learning or change in line with the aims of the project.

It perhaps deserves emphasis that, in presenting the above characterizations of the attitudes and degree of motivation of the staff, we are trying to state what we think was an important and obvious ingredient in the intervention in the lives of the children. This ingredient was less a variable than anything else subsumed under the term "intervention." To have omitted presentation of this ingredient, as is too frequently done, would render it impossible for the reader to assess the quality or the flavor of the intervention which, after all, was primarily interpersonal in nature. It is possible for us to describe, and for the reader to comprehend,what we did. It is more difficult, but no less important, to give some idea of "how it was done."

Scheduling and Teaching Staff

During the first school year, the laboratory school was in double session for 166 days. Classes were held five days a week for three hours each session. The morning classes ran from 9 a.m. to 12 p.m. and the afternoon classes from 1 p.m. to 4 p.m. In the morning the pilot study group occupied one of the two basement classrooms. Two other groups occupied the first floor classrooms.

In the afternoon both of the basement classes were used by the project children, but only one of the first floor classrooms. This arrangement was mainly a matter of convenience. All told, six classes were held each day, the enrollment ranging from six children in the smallest class to ten in the largest.

As was indicated earlier, all of the experimental children were transported to and from the project each day by two school buses. One bus brought the primary sample children and the other the pilot study group,who came from a different community. The distances traveled to the laboratory school were approximately eight and five miles, respectively.

During the second school year, the laboratory school was in single session for 147 days. Classes were held four days a week for three and one-half hours each session. On Fridays, the entire project staff met for three hours in order to staff children and deal with curriculum problems, as well as administrative matters. There were several developments that permitted reducing the number of classes participating in the project. First, by this time all of the pilot children were attending public school kindergarten or first grade. Secondly, the few experimental children that no longer attended our school--either because they failed to attend or moved out of the area--reduced our population sufficiently to warrant a decision to conduct all classes during a one-half day session. As a result, during the second year all classes were held from 1 p.m. to 4:30 p.m. Children were divided into two groups of 18. Each group was assigned one head teacher and two assistant teachers. Busing arrangements were essentially the same as in the previous year, with the exception that the pilot study group no longer attended the school.

The teaching staff was divided into three categories: the head classroom teacher, the classroom assistants, and the booth teachers, or, more strictly speaking,the "booth assistants." Moore (1960), has made the point that because these adults do not teach in the usual sense in which the word is employed, the term "booth assistants" is preferable.

During the first year, there were four head teachers. Two were male and two were female. None had previous teaching experience with preschool children; however, three of the teachers had many years of experience teaching special classes for the mentally retarded, and one had several years of experience teaching deaf-blind children in an institutional setting. Three of the teachers held the Master's degree and one of these is currently a candidate for the Ed.D.

During the second year, there were two head teachers and four assistant teachers. One head teacher, a male, had served the previous year as head teacher. The new head teacher, a female, was an experienced nursery school teacher, joining our project after having worked several years with preschool mentally retarded children.

Four booth assistants were supervised by a staff member who was trained at Moore's Responsive Environments Laboratory expressly for this purpose. To guarantee that Moore's methodology would be adhered to as closely as possible, extended visits were made by one of the authors to this same laboratory, prior to the initiation of the present study. Additional visits were also made by other key personnel during the school year.

All of the booth assistants were graduate students at Boston University, and most of them were studying in the area of Special Education at that institution. One of the four assistants was a male.

In addition to these members of the teaching staff, a music teacher was hired to provide instrumental and/or choral instruction for each class on a once-a-week basis. Oftentimes, a student volunteer (an undergraduate), was able to work for a period assisting in the management of the children on the school buses or helping out in the classroom.

The Other Adults

From the previous sections, one might conclude that a child in one of the classrooms had the possibility of interacting with several adults: his teacher, the booth assistant if he were in the typewriter group, a music teacher, University students, the bus driver, and the bus assistant. In fact, there were many more adults in the laboratory school whom the child saw,and could interact with,in varying degrees. For one thing, there was an almost complete turnover of teachers and booth assistants at the beginning of the second year. In other words, over a two-year period any one child had developed close relationships with two sets of adults, a degree of experience with the problems of separation, affiliation, and social adaptation probably far beyond that experienced by the children in the non-experimental group.

The school setting we are describing also served the purpose of preparation in the manner described elsewhere by Sarason, Davidson, and Blatt (1962). Briefly, the aspect of teacher preparation conducted in this setting, involved several groups of Boston University undergraduate and graduate students in an observation seminar: students observed a classroom through the one-way mirror and then had the opportunity to discuss their observations with faculty members who also had observed the children. The children were aware of being observed and on several occasions were invited into the observation room while a seminar group was there. We assumed that the children would be curious about the room and the people they could see going in and out. The physical layout was such that it made no sense to attempt to hide anything from the children, even if one were disposed to do so. We do not want to create the impression that there was a lot of interaction between the children and those in the observation seminar. We simply wish to state that these college students were part of the setting perceived by the children and some interactions were possible and did take place.

The crucial point deserving emphasis here is that the children experienced a variety of interactions with a fair number of adults who were aware of their responsibility to handle their relationships with the children in a manner consistent with the overall aims of the study. Put in another way: these children experienced, over a two-year period from a variety of adults in an educational setting, responsiveness, friendliness, support, help, and stimulation. This is not to say that, while in the setting, the children were always happy with, free of frustration from, and in a harmonious relationship with adults. It is to say, however, that it seems permissible to conclude that these children experienced their interactions with adults in a manner reinforcing of attitudes that facilitate learning in the school setting.

Supervision and Staff Interaction

The writers, each in varying degrees, supervised the teaching personnel. We were quite aware, both before and during the study, that we would be dealing with a relatively complex social-psychological setting involving child-child, teacher-child, teacher-teacher, parent-teacher, parent-child, and supervisor-teacher relationships. When one brings together a group of professional people who vary in personality, background, and outlook, it would be an instance of optimism run rampant to expect that there would be no interpersonal friction or disagreements as to procedure and orientation. It was our job to become aware of these problems when they arose and to meet them as best we could. As one might expect, interpersonal friction and disagreements about procedure occurred more frequently in the beginning months of the project than any time thereafter. Although we are obviously biased in this respect, it is our opinion that these problems reflected more the seriousness with which people viewed their role in relation to the overall project, than it did anything else. All this is by way of reiterating that this project consisted of people to whom the success of their work with the children was of great importance. They acted as professionals, not as mere job-holders.

Perhaps the most important function of our supervision stemmed from our role as observers of teacher-child interactions, either by direct observation or by listening to a teacher's presentation and discussion of the behavior of the children. In these ways, we saw problems about which action on our part was indicated--action which has to be subsumed under the term "intervention." We give below, a series of examples illustrating the role of the supervisors in this project.

Throughout the intervention years of this project, active collaborations were maintained between all teaching staff and supervisors and consultants. Weekly meetings were held, generally lasting from two to three hours, in which teachers were asked to present any questions or problems that they felt were important to discuss. We were most interested in providing a setting where teachers could talk about those things which were most threatening to them and, therefore, most difficult for them to discuss. For example, we tried to make very clear our belief that it is impossible for a teacher to like every child in her class equally well. In fact, there are conditions that sometimes cause a teacher to actively dislike a child in her class. We were very anxious to communicate to teachers the need for them to discuss such children. In certain clinical settings, teachers are discouraged from giving opinions about children whom they do not like, or are otherwise prejudiced about. These children, we felt, required our greatest attention. To the degree that teachers are unable to verbalize their prejudices or discontent, they are unable to resolve them. We felt that the inability of teachers to discuss threatening situations and conditions was the most serious roadblock to their solution.

The development of our preschool curriculum was accomplished through frequent collaborations among the teachers, booth assistants, and supervisors. These collaborations centered on the study of individual children and from this study emerged a curriculum. Naturally, both the teachers and supervisors had certain strong biases concerning both our objectives and how we might best accomplish them. The expression and testing of our various positions in the study of individual children permitted some rapprochement of one point of view with another and, thus, clarified what we were attempting to accomplish and in what direction we were heading. The purpose of all of the above was to narrow the almost unavoidable discrepancy between what we wanted to do, and what we did. If we did not accomplish that objective as well as we might have wished, we feel that our collaborations in jointly studying children and developing curriculum aided us in more satisfactorily understanding whatever was done.

Implicit in our intentions as supervisors, were our active efforts in establishing a setting whereby teachers might openly communicate their feelings of satisfaction, dissatisfaction, puzzlement, and ignorance. We, in turn, reserved these same opportunities for ourselves--and frequently took advantage of them. Further, we felt a strong responsibility to meet with teachers at every opportunity, to discuss and analyze our strategies and tactics, and to share with them the multitude of problems and burdens that are attendant with any large field study. There is no way we know of to provide data supporting our belief that, in this study, teachers and supervisors were actively involved with each other in meaningful professional relationships in an atmosphere of openness and mutual trust, other than to say that we believe strongly that this environment was present.

The Parents

Up to this point, we have largely discussed the attitudes and activities of the staff as aspects of the intervention. As it turned out, there is justification for discussing parental attitudes in much the same way. There is no doubt that we viewed our project as an attempt to counteract and/or compensate for conditions of learning and living in the home. It is also true, however, that we were aware of the possibility that the process of intervening into the lives of these children in the preschool setting might produce certain changes in parental attitude and behavior. Although everyone now recognizes that parental behavior influences children, it is too often overlooked that changes in children can produce changes in parental attitude and behavior. We did not know whether this would, indeed, happen in our study but we were set to note anything which suggested that it might be occurring. On the basis of what follows, it would be misleading if we were to conclude that, whatever changes may have occurred resulted only from what happened in the physical confines of the project setting.

Indications of the attitudes of parents were gleaned from five principal sources during the course of the study. These sources were as follows: (1) scheduled parent visitation days (open house for parents); (2) unscheduled visitations to the project by parents, i.e., "dropping in" without notice; (3) home visitations by the associate director and/or the supervisor of booth instruction; (4) conversations of parents with the teachers assigned to bus duty; and (5) telephone conversations. These five sources of attitudinal information will be discussed in order.

Parents' Day Visitations. One day was scheduled in the fall for visits by parents and one in the spring of each school year. Because the project was not readily accessible by public transportation from Cambridge, parents who could not provide their own transportation, for one reason or another, were invited to come to the project on the same bus used to transport the study children. Most of the parents who came availed themselves of this opportunity, although a few furnished their own transportation.

It might be mentioned, parenthetically, that the state institution for the mentally retarded, where the present study was conducted, is located a distance of approximately eight miles from the community providing the primary sample. Furthermore, the parents continued to send their children to the project school, despite the fact that the school was situated in an institution for the mentally retarded. That this was a definite factor coloring the reaction of the parents to the project school was evidenced in several anxious, telephoned inquiries.

Although many, if not most, of the parents learned that the project school was housed in an institution for the retarded, this fact did not deter them from sending their children to the school. Several anxious parents only wished to be reassured that the project school was not expressly organized for retarded children. The study parents were as sensitive to the stigma of mental retardation as are the parents of children from higher social classes, although not to the degree where they would deny their children the benefits of an educational program merely because it was located in close proximity to large numbers of retarded children.

About one-third of the parents turned out on each of the two visiting days the first year. More than one-half the parents attended during the second year. Considering the distance traveled, the transportation difficulties, and the fact that many of the study children have working mothers, this was felt to be a surprisingly excellent showing.

Remarks made by parents on these two occasions during informal conversations with staff members indicated clearly that they were enthusiastic about the program in which their children were enrolled. During the first year, inquiries were made by them concerning the enrollment of their children in the second year of the program; anecdotes were related describing various positive changes in the behavior of the children and, more importantly, perhaps, it was pointed out that many had been contacted by neighbors who had heard about the program and who wished to enroll their own children.

These reports were supported by the numerous telephone calls made to the project by many of these same neighbors, requesting admission of their children to the program. It is interesting to note that several of the parents of non-experimental children made similar requests. It was also significant that a large majority of parents of experimental children expressed the wish to have the program continue beyond the stipulated period, so that younger siblings of study children could receive the same perceived benefits attributed to the program.

It would be naivé to deny the very real possibility that the parents may have suppressed negative reactions in the presence of the staff; however, it must be said that, at least overtly, the behavior of the parents could only be characterized as very friendly and relaxed.

Perhaps one of the most solid indications of a positive attitude of the parents toward the educational program was that, with one exception, every parent who had the option of enrolling his child in a regular public school kindergarten at the start of the second year of the project school or of re-enrolling him in the project school, elected to do the latter. Again, it is apparent that any stigma of an institution for the mentally retarded did not constitute a serious obstacle to the program.

Unscheduled Visitations. Except under unusual circumstances, visits to the project by individuals other than qualified professionals,with a legitimate interest in this type of research, were discouraged. The impromptu appearance of the parent of a study child at the project is an example of one of these "unusual circumstances."

Occasionally, during a school year, a parent (a father in most instances) would "drop in" to chat with the associate director and inquire about the progress of his child. Usually the fathers were taxicab drivers or truck drivers whose trips sometimes took them into the vicinity of the project. Because there were few opportunities to contact working fathers, this practice was not discouraged.

Invariably, the parents making these impromptu visits would express their unsolicited gratitude for what they felt was being done for their children in the program. When asked in what ways they thought that the behavior of their children had changed, they cited such things as improvements in speech and language development, the correct reproduction of little songs learned in school, the development of desirable personality traits, the ability to print letters (sometimes a child's entire name), etc. One parent explained that her son no longer ate the letters in his alphabet soup, but read them! Often, the parent asserted that the study child either compared favorably with, or outperformed, in certain school-related skills, an older sibling now attending first grade.

Frequently, these and other parents referred to their study child or children as having become "smarter," since entering the project school. Parents also commented on how glad they were that their study children were receiving this "break" in school, a break which they felt they themselves had been denied.

Home Visitations. As previously mentioned, home visitations were made only by the associate director and the supervisor of booth instruction. In the second year of the project school, however, this restriction was lifted so that the two full-time head teachers could also make visits to the homes of the study children. This shift in policy was made, chiefly, because it was agreed by the staff that there was an increasingly urgent need to obtain additional information on individual children who were presenting special problems in discipline and in mental and physical health.

Home visitations were generally made, in the first school year, to find out why certain children were not being sent to the project school, and to persuade the parents to send them. Some of the parents were influenced by the acting out behavior of their children, i.e., they would be fearful of precipitating a tantrum or hysterical vomiting in the child and would yield to him. Other parents simply accepted the child's pronouncement that he did not wish to go to school and kept him home. Families moved away and families were burnt out of their homes (eight in the first year of the study). A few parents refused to send their children unless the bus teacher escorted the child from the home to the bus stop and from the bus stop to the home. Seldom was a child kept at home because of any articulated negative attitude toward the project school. It is important to note that no effort was spared to encourage parents to keep sending their children to the project school. If needed, clothes were purchased for some of the children, and in one instance a private investigator was retained to trace two study children who had apparently left the state with their families.

During these visits to the homes of the children, parents of other children in the study were sometimes encountered and pleasantries exchanged. Positive reactions toward the program were revealed in these informal contacts, not only in the cordial behavior of the mothers and fathers, but in the anecdotes they related concerning the "improvements" in the social and intellectual behavior of their offspring.

Conversations of Parents with Teachers Assigned to Bus Duty. Occasionally, parents would communicate grievances concerning the project (mostly relating to the arrival or departure time of the bus on certain days at their pick-up points) to the teacher on bus duty. These were duly noted and conveyed to the associate director. Bumps on a child's head, perhaps received on the bus, lost mittens, damaged clothing, etc., all fell into this category. These complaints were usually resolved with little difficulty. The bus teacher, it should be noted, was furnished each day with a prepared form on which he recorded remarks by the parents and any unusual behavior or comments of the children riding the bus.

Telephone Conversations. All phone calls were handled by the associate director and the supervisor of booth instruction. A form was also devised for recording the content of these conversations and the identity of the caller. The parents were always dealing directly with the two individuals who could give them immediate answers to their questions. There was never any necessity to deal with a "secretary," or some other intermediary in matters related to school policy. This technique was found to be very successful in nipping rumors in the bud which might prove ultimately harmful to the study. For example, at the beginning of the first school year, some of the parents of study children were told by neighbors that the project school was for retarded children only. Naturally, the parents were alarmed; but fortunately their fears were quickly dispelled through this method of providing direct and authoritative information and the rumor was eliminated. Parents were also erroneously informed that their children would not be able to attend first grade, without first repeating kindergarten, if they elected to enroll their children for a second year of preschool. Despite letters to the parents from the school previous to the appearance of this rumor, some families had to be reassured by telephone that such was indeed not the case.

Several parents called to thank the staff for the "improvements" that had occurred in their children's language development, toilet habits, interest in books, etc. A few complained that their formerly shy child had become too outgoing and was acting "fresh" at home. Whether or not such behavior was necessarily undesirable from the standpoint of the child's mental health, was difficult to determine. Finally, it should be mentioned that some parents communicated their positive sentiments toward the project in short notes and letters, and through occasional greeting cards.

What we have presented in this section suggests that changes in the children that may have occured over the two-year period should not be viewed as being solely due to what was going on at the project school. We cannot say how much weight should be given to the parents as interveners. It does seem as if what was going on in the school was having some kind of effect at home which, in turn, affected the child in relation to the school.

We have attempted in this chapter to set forth those aspects of the "psychological climate" of the study which we feel must be considered as part of the intervention. It should be apparent, that it is in the nature of things that the term "intervention" refers to a number of related variables which affect each other in subtle and complex ways. We make no claim that we can differentially assign weights to the different aspects of the intervention. We have opinions in the matter which will be discussed in later chapters.

In the next chapter we take up the more easily described aspect of the intervention: the formal curriculum. In that chapter, the reader will find described,in some detail, the contents and goals of the curriculum: materials used, tasks assigned, skills learned, and over-all objectives of the daily routine.

Chapter VI

The Curriculum: Content and Aims

In the classroom situation our objective was to provide what can be termed an optimal preschool environment. To achieve this aim, considerable time and energy was invested to create an environment which, it was felt, should be effectively rich and stimulating, rather than objectively rich and stimulating.

Over and above this goal we sought also, through much trial and error exploration in methodology, to provide certain structured pre-academic experiences which we thought might help to develop readiness for the academic activities formally embarked upon in first grade. The extent to which this type of activity was emphasized in our nursery program, and in the Responsive Environment, is the extent to which our total educational program may be said to differ from conventional preschool or nursery school programs.

Rather than wait for readiness for academic learnings to develop, we strove actively to develop it. We tried to achieve this end in three principal ways: (1) by helping the children learn how to function socially in a group instruction situation so as to be maximally receptive to that instruction; (2) by providing a concentration of experiences designed to arouse curiosity and to promote attitudes of inquisitiveness and positiveness toward learning; and (3) by deliberately attempting to provide training in certain psychological functions generally considered to be fundamental to the later acquisition of academic skills in the primary grades.

A variety of games and activities were employed by the head teachers to achieve the three objectives enumerated above, some of which are described below. Each of the teachers, it should be noted, tended to stress certain types of experiences in his or her class more than the other three; however, there were also certain unmistakable commonalities in the approaches of each, both in content and in philosophy.

Teacher A, for example, emphasized what might be termed more formal content. In addition to furnishing much practice in writing and recognizing letters and in the development of quantitative concepts, she often taught lessons which were aimed at instilling rudimentary notions of causality relating to observable natural phenomena in the child's world. In simplified presentations, she dealt frequently with such phenomena as evaporation, combustion, protective adaptation, the growth process in plants and animals, electricity as a source of heat, magnetism, and weather.

The following is an actual and typical lesson plan as set forth by Teacher A:

The Evaporation of Water through Boiling

I. Objectives

A. To teach that heat causes water to turn to a vapor and go into the air or atmosphere.

B. To teach the following vocabulary words: hotplate, pan, little bubbles, big bubbles, boiling, water vapor, evaporation, air, atmosphere, plug, electricity, current.

C. To help the children learn to watch a process quietly for some minutes.

D. To teach safety in regard to heat.

E. To teach electricity as a source of heat.

II. Motivation

A. Where does the water go? (Watch and see; it is going to go away)

III. Materials
  A. Table
  B. Electric hotplate
  C. Pan
  D. Water

IV. Procedure
  A. Materials explained and introduced.
  B. Child plugs cord into wall socket.
  C. Redness of coils observed and progressive rise in temperature noted.
  D. Small bubbles, larger ones, finally boiling observed followed by gradual disappearance of quantity of water.
  E. Water vapor noted as disappearing into the air.
  F. Children asked to repeat associated vocabulary words.

V. Results
  A. All children observed the various steps of the process fairly well (five or six in the A.M. group intently interested), remainder of group watched intermittently but were interested.
  B. All five in P.M. group watched intently.
  C. All closely observed empty pan.

VI. Evaluation
  A. Active visible procedures held child's interest better than the telling of a story.
  B. The attention was good with some, excellent with others.
  C. This lesson will be repeated several times to reinforce vocabulary, procedure, and for testing of individuals.

This lesson plan serves to illustrate two of the factors common to the instructional methods of all project teachers: (1) correlated learnings were included in virtually every lesson (e.g., safety, vocabulary concepts, electricity as source of heat in above lesson); and (2) all of the teachers found that "active, visible procedures" were more attention-sustaining than a mere story or a verbal description of a process. These observations are, of course, in complete accord with the thinking of many educators who have long advocated the use of concrete, first-hand experiences, particularly with very young children.

Teacher B tended to center her planning around artistic and dramatic themes, although there was often a considerable overlap of her specific objectives with those of the other teachers. Children in Teacher B's class were frequently called upon to draw, paint, cut and paste, and act out roles of characters in stories told by the teacher.

The following plan is illustrative of some of the more structured aspects of her program. It demonstrates her interest in developing such psychological factors as attention span, color concepts, and the child's awareness of his own body image. Most interestingly, it points up the difficulty of undertaking a fairly involved instructional activity with too large a number of very young children. There were, on this particular day, 14 in the A.M. group as opposed to six in the P.M. group.

Arthur and the Pumpkin-man

Estimated time: 30 minutes

Subject: A story, followed by the assembling of a "pumpkin-man" (paper)

General Objectives: A. Lengthen attention span

B. Increase vocabulary

C. Increase knowledge of colors

D. Familiarization with parts of body

E. Increase manual dexterity

Specific Objectives:

A. Work on gaining attention of all children through Halloween story using familiar Halloween figures

  a) pumpkin

  b) cat

  c) bat

  d) witch

  e) ghost

  f) scarecrow

B. Increase vocabulary by above words (a-f).

C. Increase knowledge of colors with introduction of orange (pumpkin) and black (arms and legs) and review of old colors: red (nose), yellow (eyes), and green (stem).

D. Familiarize with parts of body: two eyes, a nose, a mouth, two arms, and two legs.

E. By pasting eyes, stem, nose and mouth, they will learn positions and by inserting tacks at joints of arms and legs they will increase their manual dexterity.

Materials:

A. Blackboard and chalk

B. Individual cut-outs

  1. pumpkin

2. two eyes
3. one nose
4. two arms
5. two legs
6. one stem

C. Four metal fasteners

D. Paste

Procedure:

A. Tell story illustrating with stick figures at blackboard

B. Assemble pumpkin (each child)

1. Paste on eyes
2. Paste on nose
3. Paste on mouth
4. Paste on stem
5. Put fasteners through joints

Learning Outcomes:

A. Familiarization with five colors: red, yellow, orange, green and black

B. Help familiarize with Halloween figures

C. Increase vocabulary by: pumpkin, cat, witch, scarecrow, bat, ghost

D. Increase manual dexterity

E. Develop pleasure from working on and completing project

Comments: A.M. Group--It was impossible to gain their attention long enough to tell the story this morning. They were particularly active and could not be quieted enough for a story. Will try again tomorrow.

P.M. Group--I felt that in general it was very successful. All the children were interested, including Jay! [Jay is youngest child in study.] They all maintained interest in pasting the face and attaching the arms and legs. Jay and Joe had to be coaxed, but finished. (Time of lesson: 55 min.)

Learning Outcomes:

A. Tommy is the only one who has a color concept. The others haven't grasped it as yet.

B. They listened intently, so I feel something has "soaked" in.

C. Tommy was the neatest, but had to be coaxed to stick it out to "arms and legs" stage. William pasted his own, then tore eyes, nose, and mouth off. However, he let me paste new set on. William also understood how to attach arms, but couldn't open hook himself. They all got the idea of spreading fastener, once prongs were started. They all need a great deal of practice in pasting.

D. All were delighted with finished product and took it home with them.

Teacher C conducted what can probably be most accurately described as the most conventional class among teachers. Relatively little that could be called divergent or creative was present in the activities the children engaged in. Neither was the variety of experiences nearly as pronounced as in the programs of the other teachers. A routinized, repetitious sameness characterized the activities in this class and there was a conspicuous absence of the experimental approach with the wide range of techniques and materials that typified the other three classes.

Teacher D proved to be unusually artistically creative and extraordinarily sensitive to the needs of the children in her class. Although all of the teachers, as was indicated heretofore, could be described as being generally more permissive than authoritarian, this teacher possessed that rare blend of understanding and firmness which makes for good class management and a minimum of "discipline" problems. This teacher also exhibited an inventiveness and ingenuity in devising specific directed exercises for developing different psychological functions that elicited eager participation upon the part of the pupils.

The following simple lesson in visual discrimination training exemplifies the kind of exercise mentioned above.

Objectives:

A. To develop visual discrimination.

B. To develop the meaning of the words "larger" or "smaller."

C. To determine the ability of each child in recognizing the size of of two like objects.

Materials: A bag of objects (like objects, but differing in size)

1. two cows
2. two rocks
3. two sticks
4. two horses
5. two dolls

Motivation: "What do you think we have in the bag? Reach in and pull something out." (All five objects have been recognized previously by all the pupils.)

Procedure: Have one child reach into the bag and pull out an object. Take out the object like one already out. "What can you tell me about it?" (Name the object.) "Which is larger?" "Which is smaller?" Vary the two questions as each object is discussed.

Results: Three out of the five pupils did a fair job on the activity. C.Y. and C.E. had very little trouble recognizing the difference between the two objects. G.L. was able to do four of the six items, making errors on the cows and the horses. L.H. (absent for past four weeks) failed to recognize any of the objects and did not understand the object of the lesson. She returned both each time. R.B. merely repeated the last word each time, e.g., "larger" or "smaller." She made no correct responses.

Evaluation: This lesson pointed out the need for further work in this area. It will be continued with objects which the children can recognize easily, so that the problem involved is not one of recognition but of difference in size of the two objects. The idea of using objects that the child can hold and look at seems to be a good one. It is evident that this lesson should be pursued further, but with like, rather than unlike, objects.

Time Allotments for Activities

Certain activities were common to all of the classes. For example, there was a toileting and general organization period immediately after the arrival of the children at school, an outdoor recess, a milk and cookies snack period, and a "clean up" period preparatory to going home. A rest period was optional, some teachers feeling that no such period was necessary. Typical morning and afternoon schedules were as follows:

Morning Session

9:00 to 9:15	Organization--bathroom
9:15 to 9:30	Story: The race between Tommy Turtle and Billy Rabbit

9:30 to 9:45	Dramatization of above story
9:45 to 10:15	Writing at tables, and on blackboard. Looking at and choosing books
10:15 to 10:30	Clean up
10:30 to 11:00	Outdoor recess
11:00 to 11:15	Milk and cookies. Speech and vocabulary work in conjunction with snack
11:15 to 11:30	Counting
11:30 to 11:40	Game--following directions
11:40 to 11:45	Clean up

Afternoon Session

1:00 to 1:15	Paint fingernails (for typing children)
1:15 to 2:00	Story and book time
2:00 to 2:30	Board time and music time
2:30 to 3:00	Outdoor play
3:00 to 3:30	Milk and cookies Numbers, weather and place concepts
3:30 to 3:45	Clean up
3:45 to 4:00	Don clothes to go home

In addition, during the course of the first full intervention year, four field trips were made--to a zoo, an animal and bird sanctuary, a commercial fishing wharf, and to the annual Christmas display on Boston Common. It was felt strongly, by all the teachers, that such excursions were important ways of compensating for certain obvious deficits in the experiential backgrounds of the children.

Needless to say, a vast amount of social learning occurred over the months during such periods in the schedule as "organization--bathroom," "clean up," "recess," and "milk and cookies." Some of the children were not toilet-trained when they first started the program and most knew nothing about taking turns, either at play or in a dining situation. Only through certain established routines in the daily program and through the continuous enforcement of consistent rules for the group were successes achieved in social learnings for the majority of the children in the study. Of course, the creation of desirable attitudes and habits were a part of all activities in the program and must not be viewed as being restricted to particular "periods" in an inflexible schedule.

A Note of Caution about the Significance of the Curriculum

It is perhaps appropriate at this point--having just outlined certain aspects of the school program and presenting, in the next section, certain important areas of concentration in the curriculum--that we say something about our concept of a curriculum.

Describing a curriculum is not a very difficult job. What is involved in such a description is a statement of specific and general goals, the means and media utilized, and the criteria for determining progress in learning. There are at least two, somewhat disquieting, characteristics which descriptions of most curricula share. The first of these characteristics is that they all seem above reproach, i.e., they seem to be described in such a way that a stand against them puts one on the side of sin fighting virtue. Criticism is further made difficult because each curriculum can point to and describe successes (just as any school of psychotherapy can point to its cures).

The second characteristic which descriptions of most curricula share is that they unwittingly convey the impression that the curriculum is a separate variable, i.e., it has an existence and effect independent of the social psychological setting and the person employing it. Such independence never exists. On paper it is possible to compare curricula independent of the settir and teacher; in practice it is impossible. To attribute consequences to a curriculum, therefore, is to do more than oversimplify. It is to misrepresent the external reality.

The two characteristics which descriptions of most curricula share not only should serve as a caution to the reader as he evaluates what has preceded. and will follow this section, but should also serve to underscore, as the following quotation suggests, the significance of a methodological problem which has received surprisingly little systematic study:

> At the present time, we simply lack the kind of detailed description of "live" teaching by means of which we can gain a better understanding of what the different protagonists in the controversy actually mean and the degree to which their descriptions are consistent with stated aims. One can point to other fields where issues were greatly clarified and productive research initiated after systematic descriptions of the live situation became available for study. Until a great deal more of these kinds of systematic descriptions of classroom learning situations are forthcoming, it will be difficult to proceed to the scientific study of the issues involved (Sarason, Davidson & Blatt, 1962, pp. 119-120).

We would reiterate that the successful development of a methodology for recording and analyzing the teaching situation is a prerequisite for any attemp to evaluate, as in the case of our study, the effects of an educational intervention. Our description of a curriculum (previous section) or areas of concentration (the next section) should not be taken as a description of a variable in the usual sense. It should be viewed in relation to our attempt to communicate the overall social psychological climate, the variations in teacher personality, and the degree to which the focus of project personnel was on the process and content of school learning.

Areas of Concentration

An overview of the total program, based on observations of the teachers and children, points up the fact that certain categories of experiences or activities, or areas of concentration, were common to each of the individual programs. These were areas which received special emphasis throughout the year. The following is a description of these areas.

1. Language Development. The results of our testing on the Illinois Test of Psycholinguistic Ability suggested that our subjects may be significantly deficient in language abilities. These abilities are, of course, essential for communicating with the other children in the class and with the teacher. The importance of these abilities in the reading process cannot be underestimated. Persistent efforts were made by all the teachers to develop the speaking vocabularies of the children and to assist them in grasping a greater number of verbal concepts. The children were constantly encouraged to express themselves in a clear and coherent fashion. Many children, who did not speak at all, or only grunted at the beginning of the program, progessed very rapidly in language development in response to the methods used. Examples of these methods are:

a. Encouraging talking and self-expression through the use of puppets manipulated by the children.

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b. Naming items drawn from a bag or a box.

c. Field trips as a basis for language experiences provided in follow-up lessons.

d. Performing a sequence of actions, dictated by the teacher, in proper order and then describing to the class what was performed.

The language development program was basic and central to the total enrichment curriculum for the children in our preschool. While there were formal language development activities, the objective of "language development" permeated all instructional areas. There were several good reasons for this. First, language is an intimate part of all curriculum activities and, secondly, there are a number of specific and highly related factors that together comprise a totality called "language." For example, the typical end product of language--speech production--derives its effectiveness, preciseness, flexibility, and strength from such general factors as language awareness, language patterns, vocabulary enrichment, experiences with stories and books, as well as from such considerations as ego strength, impulse control, and values. Such specific factors as auditory discrimination and memory, speech training, and motor coordination affect speech production. Therefore, it is not difficult to predict a strong relationship between language development and cognitive development, raising again the question, "Do children who have high IQs develop early and rich language patterns or do children who are afforded an early and stimulating language environment develop high IQs?"

What are the consequences of an impoverished language development, other than how language development relates to measured intelligence? Language, certainly, is essential for communicating with others in the classroom, at home, and in the community. Further, as mentioned before, the relationship of language to reading is very strong and direct. Therefore, the development of an adequate verbal communication system appears to aid in the general development of cognitive abilities, in communicability between individuals, and in preparation for reading and other academic aspects of the school curriculum.

2. Auditory Discrimination. The ability to discriminate sounds and to listen is essential in both reading development and speech production. Disadvantaged children often come from homes that are not only non-verbal but, at the same time, they are noisy and crowded. As a result, these children often enter school with a language that is quite different from the language they are encouraged to use in the classroom (Gotkin & Fondiller, 1965). Further, they have had an experience in coping with the noise of their environment which often results in a developed ability to "close out" sound. Therefore, learning to listen has not been a particularly well-developed skill which these children bring to the preschool.

Our objective in the auditory discrimination program was to help children develop an awareness to sounds and their components, the recognition and identity of sounds, the identification of likenesses and differences in sounds, the reproduction of sounds, following directions, acquiring information, recognizing relationships, enjoying and appreciating spoken language and music, and attending to speech and other meaningful sounds. There were a number of excellent activities our children engaged in that aided in the development of auditory discrimination. Two of the more commonly used ones were:

a. Recordings of animal sounds and musical instruments were played. The children were assisted by the teacher in recognizing these sounds.

b. Sound-blending games were played. For example, "What is this—sh-oe?"

(Teacher sounds word with little break between, then increases to clear separation of sounds.) Many of the children could not make some of the sounds accurately, but they could recognize them auditorally.

3. Auditory Memory. Related to the above section, both in objectives and program activities, specific preparation was given children to develop the ability to remember things heard:

a. A story was read to the children; then they were encouraged to retell the story in their own words. Sometimes they were asked to repeat statements made by characters in the story.

b. Children were given verbal messages to convey to other staff members. A check for accuracy was then made.

4. Visual Discrimination. The ability to discriminate visually between letters and words is essential if a child is to learn to read. The children in the experimental-typewriter group were continuously receiving practice in this skill, because of the very nature of the activity in which they were engaged in the booths; however, these children, and children not on the typewriter, were given additional visual discrimination activities in the classroom situation. Two of these activities are listed below:

a. Superimposing letters and words printed on transparent plastic squares onto identical letters and words printed on a "Bingo" card. This proved to be one of the most popular games among the children, probably because of the "immediate reinforcement" feature of seeing the exact match at once and because of the remote chance of making an error.

b. Size and shape discrimination employing toy animal figures and other objects (see teacher's lesson plan, pages 110-112).

5. Visual Memory. Training was furnished in the ability to remember things seen. This skill is related to reading and spelling.

a. Objects were placed on a table before the children. Then, while their backs were turned, the teacher either removed or covered an object. The children were asked to identify the absent object.

b. A sequence of related pictures was placed before a child in correct order. After a few moments, the child closed his eyes. The pictures were arranged in an incorrect order by the teacher. Then, the child was to rearrange them correctly.

6. Other Sensory Modalities. Experience was provided in the following sensory areas: touching, smelling, tasting, and, for want of a better term, kinesthetics (utilizing a variety of sensory functions, the child's conscious perception of his own muscular movements). Tactile (touching) activities might include describing the "feel" of surface textures, comparing textures, classifying objects according to texture, and, eventually, relating texture characteristics with classification of materials.

Olfactory (smelling) activities included relating odor to its source (e.g., vegetables, smoke, grass), noting similarities and differences in odors, describing various things according to odor, and then classifying objects according to odor. Such natural activities as cooking together, snack time, sampling food, and walks in the woods brought meaning to this aspect of the program.

Tasting activities included learning to describe and compare different tastes (e.g., sour and sweet, bitter and bland, pleasant and unpleasant), classifying objects according to taste, and relating taste to its source.

Kinesthetic activities helped children develop concepts of space, size, movement, and coordination. Such activities as tracing shapes and sandpaper letters, closing eyes and tracing letters in space, and closing eyes and relating one part of the body to another provided good kinesthetic training for the children.

7. Quantitative Thinking. Four major areas of growth were attended to in this area of concentration. These areas were: (1) the ability to understand and use the language of quantitative thinking; (2) the ability to count and understand number concepts; (3) the ability to recognize at a glance simple groupings such as 1, 2, and 3; and (4) the ability to recognize and understand written numbers.

a. In a shuffleboard-type game, the children took turns throwing a blackboard eraser into four, concentric, numbered rectangles drawn on the floor of the classroom. The highest number (four) was printed in the smallest rectangle; the lowest number (one) in the largest rectangle. The number, or score, was then recorded on the blackboard by the player.

b. Practice in counting and in recognizing small groups of objects was a frequent activity, often accompanying snack time (counting the number of cookies or napkins to be passed out), but many times being offered in specific directed exercises with poker chips, marbles, and other objects.

8. Motor Coordination. Both large and fine motor coordination activities were provided. Particular attention was given to the development of fine motor coordination, since many of the children appeared to have had limited opportunities for this type of activity in their home environments.

a. Much cutting with scissors, drawing and tracing, and construction of objects out of paper and other media was employed.

b. Numbered dots were placed randomly on a sheet of paper. The child was then required to connect these properly.

c. Walking a plank, suspended at progressively higher altitudes as a child's confidence increased, was another popular activity.

9. Speech Training. Typical classroom speech correction was given by the teachers. However, because of the ages of the children, insistence on "precise" pronunciation was avoided. Rather, the children were gently urged to develop an awareness of the need for clear, grammatically correct communication. For example, if a child said, "Me want to go to the bathroom," he was urged to say, "I."

10. Creative and Imaginative Thinking. We believed that developing a style of thinking and behaving that is divergent, unique, and creative would aid substantially in the child's ability to express himself and his feelings in a manner which is integral to his personality and expressive of his individuality--not of the stereotyped responses of "anyone." Attempts to stimulate creative and imaginative thinking were made in several ways:

a. Through sentence completion exercises, e.g., "If I had a dog. . ."

b. Through dramatic play.

c. Through familiarization and experimentation with a variety of concrete artistic media (clay, paper, macaroni, paint, etc.).

d. Through a host of commercial games allowing for construction-type activities with all kinds of materials (wood, plastic, metal).

e. Through having the children make up stories and songs for presentation to the class.

Needless to say, most of the activities and games employed in the preschool situation served to develop more than one of the functions described above.

The teachers were keenly aware of this fact and constantly strove to integrate learnings in an effective way. In many preschools, the kind of training in the psychological functions described above occurs only incidentally and is not deliberately planned. In our program, such specific training did not predominate over all other aspects of the program; however, definite periods of time during the class day were set aside for this purpose.

In conclusion, it can be said that the preschool program can best be described as experimental, emergent, and child-centered for the most part, adhering to the basic principles of any sound preschool program but, over and above this, focusing on the intensified development of pre-academic skills.

The Responsive Environment

Omar Moore (1960, 1961, 1963) has pioneered in the development of theoretical as well as technical aspects of the learning of preschool children. Because Moore's "Responsive Environment" was used in the present study, it is necessary that his position be reviewed briefly. In this review, we hope it becomes apparent why Moore's contribution to the field fitted so neatly into our efforts at environmental stimulation.

Moore has described how children aged two to five can learn to type, to read, and to write. These skills are acquired through an enjoyable experience. This experience is derived from what has been labeled a "Responsive Environment." An environment is "Responsive" if it satisfies the following conditions:

(a) It is attuned to children's exploratory activities.

(b) It informs children immediately about the consequences of their own actions.

(c) It permits children to make extensive use of their capacities for discovering relations.

(d) It is so arranged that children are likely to make a series of interconnected discoveries about some aspect of the physical, cultural, or social world (Moore, 1960, p. 4).

Moore believes that when organisms are comfortable--i.e., when their basic biological drives have been satisfied, and when they are afforded the leisure to do so--they will engage in exploratory behavior. In addition to this innate curiosity, there is also a drive to manipulate and a motive to be competent. A Responsive Environment will, therefore, permit the organism to learn under these conditions but it does not teach. It allows children to discover things for themselves, much in the way they learn their native tongue.

Moore also believes that children under five have had only limited practice in following complex verbal instructions and that most children seem to become bored when required to listen to adults in such situations. Moreover, there is no set of explicit rules which is really adequate for making the translation between speech and orthography.

In the Responsive Environment the learning is said to be "autotelic," that is, the children are to use the machine (an ordinary electric typewriter in the non-automated environment) for its own sake and not in response to extrinsic rewards and punishments. The child may err or achieve successes without incurring other consequences. The opportunity to operate in this manner is crucial to Moore's method, for he feels strongly that anxiety and fear act so as to hamper free exploration and discovery and, consequently, learning.

The four stages of Moore's system for permitting children to learn orthographic symbols have been outlined as follows:

I. Stage I - Basic Reading and Writing
   A. Steps--typing
      1. C (child) explores typewriter (electric), T (teacher) responds by naming each character C makes. (T also gives phonetic value of some characters).
      2. C gives T's response to C's making of characters.
      3. T exhibits characters on projector. C responds by striking the appropriate key and T repeats C's verbal response. (By this time, correct fingering is achieved through T's use of off-on switch).
      4. T exhibits word list on projector, C responds by typing the characters in proper order. T then pronounces the word.
      5. C makes T's response to words.
      6. T exhibits sentences on projector, C types words (including punctuation) in proper order. T no longer responds except to help when C is confused.
      7. C types autonomously. (A control is added to projector so that C can do this by himself).
   B. Steps--printing
      1. T makes blackboard and chalk available to C at the beginning of typewriter training.
      2. At some time C will begin to make letters, words and sentences on his own; T corrects mistakes after this process has begun.

C. Steps--reading

1. T presents word lists and stories (from C's previous typing) on projector. C reads, T corrects (having first given C an opportunity to work through the difficulty).
2. T provides books so that C may read autonomously.

II. Stage II - Dictation

A. T records C's verbal responses while C types, then C listens to himself.

B. C reads typed material while listening to himself.

C. T adds recording-reproducing unit with keyboard control to typewriter, as well as off-on switch for the projector. C explores these controls and T responds by naming them.

D. C reads (but does not type) prepared story on projector (reading punctuation as well), and then takes his own dictation on typewriter.

E. C takes dictation on typewriter from other recorded voices.

III. Stage III - Composition

A. C dictates his own stories, with T present, and then types them.

B. C dictates his own stories, with T absent, and then types them. T inspects and corrects C's work.

C. C dictates and types privately and is encouraged to start a diary. T does not intrude.

IV. Stage IV - Dialogue

A. T collects samples of C's verbal interaction with others. C's task is to type these recordings as a dialogue, assigning utterances to the proper persons.

B. T records interaction in contrived game situations of the kind devised by Moore (Moore, 1960, pp. 5-6).

In the present study, "instruction" was begun starting with Stage I--Basic Reading and Writing--in the five booths provided for this purpose. The booths, 7' x 7' x 7', were arranged in a line along one wall close to one of the classrooms and separated from it by a corridor. Each booth was equipped with a wall-mounted blackboard, an electric typewriter with an attached Line-a-Time paper-exposing device, a chair for the child and one for the booth assistant, and a table which could be used to support a DuKane filmstrip projector. The booths were also monitored for sound and could be observed from the outside through windows containing one-way vision glass. Each of the typewriters was equipped with an instructor's off-on switch, so that the keyboard could be immobilized at any time at the discretion of the booth assistant. The keyboard of the typewriter was color-cued with bits of colored tape in the manner recommended by Moore, the colors on the keys matching those painted on the fingernails of the children prior to entering the booth. Other than the above-mentioned items and the sound equipment (wall microphone and amplifier), the booths were bare.

Each day that school was in session, the booth assistant would go to the classroom. A child in the experimental-typewriter group would be sent out to the booth assistant who would then escort the child to a special area where his fingernails would be painted. After his nails were painted, he was led directly to the booth. (The booth assistant would record the exact time that he entered and left the booth). If the child, for any reason, did not wish to go to the booth on that day, then he was allowed to stay in the classroom. Similarly, a child remained in the booth only as long as he desired. In any event, the booth session was always terminated at the end of 30 minutes. The child was then escorted back to his room by the booth assistant and was provided with a carbon of his typing for that day which he was permitted to take home.

At the close of each session in the booth, the booth assistant immediately wrote down his observations concerning the child. A daily staff conference was also held at which the performance of each child was reviewed. In Moore's laboratory a record is kept of the amount of time spent in the booth and the number of times the child depresses keys (stroke count). For our purposes, this record was considered minimal; therefore, data over and above these were collected, e.g., the number of refusals to go to the booth, the average time spent in the booth with each booth assistant, the average time spent in the booth by the entire sample.

In endeavoring to observe Moore's procedures to the letter, as well as to the spirit, assistants were specifically instructed to be as impersonal as possible in the booth situation, so as neither to reinforce nor to "punish" any child's responses inadvertently. We wished to make every effort to promote the experience with the typewriter as truly autotelic. We must admit, however, that it was virtually impossible for the booth assistants to maintain the same degree of objectivity at all times. Moore recognizes this problem, but believes that it is now largely, if not entirely, eliminated with the introduction of the fully-automated E.R.E., i.e., the Edison Responsive Environment or "talking typewriter."

To assist the reader in forming a clearer picture of the non-automated Responsive Environment, the type used in the present investigation, a general description is given:

Miss Smith enters Jimmy's classroom and, at a natural interval, asks him to go with her to the booth. If Jimmy does not want to go with her that day, she does not attempt to coax him but again suggests that it might be fun to play with the typewriter. If Jimmy decides to go, the teacher and the child go to a desk where Jimmy has his nails painted appropriate colors, corresponding with colors on the typewriter keys. This is a good opportunity for the teacher to review the colors on the child's fingernails. The teacher notes the colors which Jimmy can name and any comments he makes in response to the colors, for example, "That's in my shirt."

Both the teacher and Jimmy then enter the booth. (The teacher lets Jimmy take the lead). Jimmy gets into his chair. He may need assistance, such as moving the chair closer to the typewriter. The teacher asks Jimmy if he would like to put the paper in the machine. Usually he does this and turns the roller to an appropriate spot so that he can begin to type. Jimmy switches on the machine without being asked to do so and he is ready to begin. The teacher has made sure that in addition to the original paper inserted in the typewriter there is also a carbon and a blank piece of paper. The second copy is later given to Jimmy to take home at the end of the day.

Jimmy is beginning the first stage of training and is, at this point, exploring the keys on the typewriter. As Jimmy strikes a key, the teacher repeats each letter, number, or other symbol. She also gives the phonic sound of each letter, with the exception of the vowels and "c," "k," "g," "j." If Jimmy is typing too quickly, she uses her control button to shut off the power and she asks him to slow down. She directs him to type with only one finger. If he does not heed this direction, she repeats the "only one finger" rule. If, however, he continues in the same manner, she switches the typewriter off and tells him that the typewriter can't work unless he slows down. Usually, Jimmy returns to one finger typing.

Using her pencil the teacher points to the letters on the paper which the child has typed. This usually draws the child's attention to the characters which he has typed and he begins to see the relationship between the keys he has typed and the characters that are immediately reproduced on the paper. The teacher asks Jimmy to repeat the letters which she is saying as he types.

If Jimmy asks questions not pertaining to the typewriter, Miss Smith replies with a short answer and then waits for the child to return to the typewriter. While Jimmy is typing the teacher takes short notes, usually of things Jimmy is saying relating to the session that day. The teacher also tries to note those keys that he uses repetitively, which hand and fingers he uses, and any new skills he acquires during the session.

The maximum length of time for which Jimmy may remain in the booth is 30 minutes per session. He may, however, leave whenever he desires before the time is up. When he asks to leave before the 30 minutes, the teacher suggests that he type some more to see if he really wants to leave. If he does wish to leave, the teacher asks him to switch off the machine and take the paper out. This he can do. The teacher and Jimmy leave the booth. She gives him a copy of the work he has typed that session and accompanies him back to his classroom.

Three general "levels of attainment," or sub-stages, can be identified in Stage I of the method developed by Moore to help children to acquire certain orthographic skills at an early age. For our convenience, these three levels have been designated as follows: (1) gross exploration; (2) differentiated exploration; and (3) integrative exploration.

Examples of behavior characteristic of the first level would be pounding the keyboard with the fists and/or forearms, or more than one finger, hence jamming the keys; manipulating various switches and levers on the typewriter, but not the lettered keys; and moving about the booth. At this level there appears to be little or no connection between what the booth teacher may be saying and what the child is doing on the typewriter.

With any given child, at any given time, there is a considerable overlap among these levels; however, there appears to be less variability within each level than between one level and the next. The variability from child to child is also great, as one would probably anticipate and as is evident from the examples of predominantly gross exploration contained in the descriptions below, written by the booth assistants, of the behavior of children on their first day in the booth:

Terry

Terry responded aggressively to the typewriter, using his whole hand and consistently jamming the keys. He moved the carriage manually and typed over the same place several times. I had to ask him to leave at the end of 30 minutes.

Beverly

She repeatedly asked, "What's this?" to various keys, parts of the typewriter, and objects in the room. She was quite interested in looking about the room. When Beverly came to the end of a line, she said, "You push them,"--meaning the keys. I asked her to leave after 30 minutes.

Jane

She seemed afraid of painting her nails but not of typing . . . She understood carriage return immediately. She was more interested in the mechanical operation of the typewriter than in actually typing. She started to use her fists, but understood when I switched her off.

Paul

Paul hit the carriage return 165 times. He pressed the lock-on--lock-off key a few times but, after the first two lines, was preoccupied with the carriage return key. He then said that he wanted to go to the bathroom. He did not want to type anymore. He did not repeat the keys after me. Occasionally, he stared at me as I said the names of the keys.

Jerry

He pressed all the keys at first, but not the "switch-on" button. He typed a row of a's and said, "a," once after I had said it. He ran his hands over the typewriter exploring various parts of it. He repeated words after me--e.g., "tab set," "carriage return," "paper release," etc. He would use the tab and carriage return, hand release, paper release, and hand return down to the bottom of the paper, and then would say "all right" or "O.K.," pull his chair closer to the typewriter and wait for me to put the paper back in. Once he said, "I'll put the paper in." Then, "I'll show you, I'll show you,"--meaning he'd show me how to put the paper in. Once I had to show him where to put the paper. He'd say, "Fix it," when the letters [keys] stuck on the carriage. He touched everything I touched and manifested great interest in my button.

The behavior of the child described below on her first day in the booth may be considered quite atypical, as contrasted with the behavior of the other subjects in the experimental-typewriter group. One is tempted to call this the behavior of an "unresponsive" child in a Responsive Environment.

Alice

Alice did not type anything. She sat there the whole time just staring

> at the machine or at her fingernails. Every once in a while, I would point to the keys and say their respective colors, and say that you play the typewriter by pressing the keys. After two or three times of this procedure, Alice looked at her fingernails and then at the keys, but said or did nothing. Then, after 20 minutes, she began to cry silently. When asked if she wanted to go back to the classroom, she nodded "Yes." She took the blank piece of paper with her. Though Alice did nothing in terms of typing, her willingness to come to the booth is one step forward. Yesterday, she wouldn't even have her fingernails painted. Today, she submitted willingly.

At the second level (differentiated exploration), the child's behavior becomes more focused and less diffuse. Particular parts of the typewriter begin to be singled out, increasingly, for attention. For example, the child may learn to operate the carriage return and persist in operating it over and over again, or the switch-on and switch-off control may be manipulated repeatedly with its relationship to the operation of the typewriter and the positioning of the material being typed finally grasped. At this level, too, the colors on the fingernails are attended to with the names of some or all being learned.

It is during this period, also, that the child begins to make the association between the colors on his fingernails and the color-cues on the keys. The keyboard begins to receive a much greater share of the child's attention than heretofore, and the names of the letters on the keys and their sounds begin to be learned. This second level is differentiated from the first primarily by the greater amount of attention given to details, both "large" and "small."

At the third level, which may be termed "integrative exploration," the child learns to combine simpler habits into more complex "higher order" units, e.g., instead of merely spacing or pressing the carriage return key, the child can now combine in correct temporal order each of the two separate motor acts. At this level, a simple three letter word may be typed and/or printed on the blackboard with the correct letters in correct sequence. Similarly, letter sounds learned in isolation at the second level can now be blended into a "higher order" unit, i.e., a whole word.

Eventually, words may be combined into the still more complex "thought" units of phrases and sentences as the child begins to read in a more meaningful and sustained fashion. The most characteristic aspect of this level of instruction is that the child is engaged most of the time in a synthetic, rather than an analytic process, although, understandably, there is overlapping again between these behaviors.

Although strictly speaking, the booth assistant strives to be as nondirective as possible, i.e., permitting the child to learn rather than teaching him, the booth assistant at all three of these levels is, within certain prescribed limits, both directive and nondirective to some degree. In general, it can probably be said that the amount of direction given increases as the child advances within each level and from one level to another.

The Complexity of the Intervention

In this and the previous chapter we have endeavored to describe the number of different factors, or variables, which comprised the intervention. In a real sense one might say that the intervention consisted of an ongoing, changing, complex social psychological setting having personal, interpersonal, educational, and cultural components. This is not the type of setting which makes description and manipulation of the component variables easy. When we were planning and organizing the study, we realized that in the event that predicted differences emerged between the experimental and non-experimental children we would not be able to say what aspects of the intervention were more or less influential in producing the results. However, we did feel that we could develop a setting (the intervention) for the experimental children which would contain elements obviously not found in the daily lives of the non-experimental children. Put in another way: we felt that we could develop and describe an intervention which would very clearly indicate that the two groups of children were experiencing such different things so that predicted findings (experimental greater than non-experimental) or contrary findings (non-experimental greater than experimental) or no findings (experimental equal to non-experimental) would be of significance.

Chapter VII

Presentation and Analysis of Data

Introduction

Data analysis in behavioral research involves a continual flirtation between ideas and levels of significance. There is an inevitable cyclical history of initially successful seduction--Hawthorne effects--followed by disenchantment--carefully designed replication. The polite and precise parameters that play such an important role in other areas of scientific endeavor turn into capricious post hoc variables that mean different things for different investigators.

In previous chapters we discussed the unfolding of our study and the subjects used in testing the validity of our strategies. In Chapter IV, we described how groups, variables, and testings were arranged in order to provide a plausible design for testing hypotheses about the effects of a two-year preschool intervention. However, as the construction of a design involves many arbitrary decisions insofar as selecting subjects, tests, and teachers, similarly, analyses of data involve manipulations which are far removed from the measurement and statistical theory upon which they rest. This is particularly true in a study that has included a nonspecific intervention (as well as a specific one), global measurements and wide gaps in our ability to control the lives of children and families, either within the experimental environment or outside of it.

These considerations are just as important in studies that find consistent group differences as in those that are either equivocal, or find that different kinds of groups are consistently similar in their behaviors. While, as scientists, we have some reservations about the data as well as their relevance, our perspective tells us that data of this study were collected as validly as possible considering the nature of field research, in general, and the special difficulties involved in studying preschool lower-class children.

The strategies and analyses of data follow directly from the discussion of Chapter IV and from the schematics (Tables 1 and 2) presented there. Sections in this chapter will be presented in order of increasing generality. In the first two sections we shall describe the original principal sample and its division into groups, as indicated in Table 1. These groups will then be compared, utilizing analysis of variance and covariance over the four testing periods. The specific hypothesis that a preschool intervention favorably affects the school performance of experimental study children will, thus, be tested.

The third section will discuss data dealing with variability across testings and will include the multiple regression analyses of the first, second, and third testing on the fourth testing variables. The last section will discuss the relationship between variables in each of the testings, with particular emphasis on the fourth testing. The more generalized focus of this section centers on an understanding of the correlates of successful school performance of lower-class children. We will specifically attend to relationships between variables: (a) relating directly to school, (b) those having to do with psychological and educational tests, (c) those having to do with non-cognitive measurements of personal behavior and, (d) those measurements having to do with the home and the family constellation.

Sample and Groups: Descriptive Statistics

The use of first testing (May, 1962) data for stratifying subjects maximized the efficiency of their random placement into experimental and non-experimental groups. In addition, those data provided a base line across groups for future testings. Table 3 lists the means and standard deviations of the scales and tests administered during the first testing. The means of the groups were all within the .01 confidence interval of the total sample mean, indicating group comparability.

The ITPA and the RORSCHACH were not included in this testing because there were an excessive number of incomplete protocols, which was not surprising in view of the ages and ability levels of children.

First, it is noteworthy that this sample of children functioned approximately seven-tenths of a standard deviation below the mean on the BINET and approximately two standard deviations below the mean on the PPVT. Secondly, the 91.4 IQ mean that the total sample obtained on the BINET is to be expected in light of other studies of culturally deprived preschool children. On the other hand, it is not possible to ascertain whether the extremely low performance on the PPVT was a result of cognitive disabilities or of inadequate standardization of the test.

While there is an unusual disparity between the two tests with respect to central tendency (Table 3), insofar as variability of BINET and PPVT and their correlation (Table 9), there are substantial similarities. If one assumes that the BINET is properly standardized for young children, then it follows that the PPVT is not accurately standardized for either lower-class children or preschool children, or perhaps both.

Other noteworthy descriptive data presented in Table 3 are: (a) the children were slightly above the total standardization mean on the Vineland Social Maturity Scale, (b) the WARNER INDEX of 70.2 for the entire sample was

TABLE 3

Descriptive Statistics* for First Testing (July, 1962)
Principal Sample (N=59)

1	2	3	2+3	4	2+3+4
Measure	Responsive Environment (Experimental) N=18	Non-Responsive Environment (Experimental) N=20	Experimental N=38	Non-Experimental N=21	Total N=59
Chronological Age (years)	3.2 .57	3.2 .60	3.2 .58	3.2 .54	3.2 .57
BINET (IQ)	92.6 13.83	92.6 11.97	92.6 12.91	89.2 13.75	91.4 13.31
PPVT (IQ)	77.0 11.91	79.0 12.21	78.0 12.11	74.9 11.15	76.9 11.88
VINELAND (SQ)	105.3 19.98	107.6 16.46	106.4 18.29	106.3 18.57	106.4 18.39
WARNER	69.2 8.36	69.8 7.02	69.5 7.71	71.7 7.85	70.2 7.83
FAMILY (Home)	1.9 .74	1.9 .77	1.9 .75	2.0 .84	1.9 .79
FAMILY (Education)	1.6 .83	1.7 .73	1.6 .78	1.8 .66	1.7 .74
Males Females	11 7	11 9	22 16	13 8	35 24

*Means are entered in upper left and standard deviations in the lower right of each cell.

well within the lower-class limits of that index, (c) about 60 per cent of the sample were males, (d) about 35 per cent were Negroes, (e) four families had two children in the project and three had three children.

The descriptive data in Table 3 clearly show the effectiveness of the randomized stratification procedures for assigning group membership. However, they say little about the representativeness of the sample. We have provided these statistics in order to facilitate comparison between our sample and those of other studies. (See Appendix, Tables I, II, III, for descriptive statistics of second, third, and fourth testings).

Effects of Interventions: Between Group Variation

Our focus was in testing hypotheses about the extent of differences between groups on cognitive ability, as measured by standardized tests as well as by teacher ratings. Of secondary interest was whether groups differed with respect to non-cognitive behaviors, including the ANXIETY scales and SCHOOL BEH.

The breakdown of the sample into two experimental and a non-experimental group presented certain problems in the analysis of data because the two experimental groups differed only with respect to whether or not they were exposed to the daily sessions in the Responsive Environment. Comparisons of the two experimental groups were, therefore, partially confounded to the extent that they did not receive separate treatments, except for the Responsive Environment. Analysis of data proceeded in several ways in accordance with the primary and secondary analytical categories of Table 1:

1. The total experimental group was compared with the total non-experimental group in order to test the straightforward hypothesis of whether the intervention produced effects.

2. The three groups were considered separate and equal groups, as they were assigned by random procedures, and the analysis was a straightforward three-group comparison.

3. Each experimental group could have been compared to the non-experimental group in a way that has been described by Dunnett (1955). However, our inability to find significant differences between groups on primary or secondary analytical categories (See Table 1) obviated this.

Another problem that presented itself concerned whether the principal dependent variable should have been a measurement of change from one point in time to another or whether it should have been a measurement of position at a given point in time (Harris, 1964). In light of the initial random assignment of subjects, either option would have been acceptable, from a theoretical point of view, but each would have denoted different (but dependent) analytical components. In terms of our data, these questions are academic because the results are consistent no matter which way the groups are analyzed and no matter which way the measurement-time problem is dealt with.

To satisfy our requirements for an exploration of the possibilities of group differences, analyses of variance and covariance were done on all of the fourth testing cognitive and non-cognitive variables. These analyses are reported in Table 4, which includes a listing of the dependent variables in each analysis and the associated classification and covariate. (Tables for each of these analyses of variance and covariance are listed in the Appendix, Tables IV-XXVII). These analyses are consistent in that they uniformly fail to reject the hypothesis (null) of no difference between groups. Thus, the evidence against differential group performance is invariant with respect to dependent variables and associated independent variables of time, age, sex, family evaluation, and intellectual level as measured by the BINET.

Table 4

Analyses of Variance and Covariance[1]

Dependent Variable	Independent Variables							
	Analytical Category for Groups[2] (First Classification)							
	Primary			Secondary				Attendance[3]
	Second Classification			Second Classification		Covariate		
	Trials[4] 2X4	Age 2X3	Sex 2X2	Age 3X2	Sex 3X2	FAMILY IV 3 Groups	BINET IV 3 Groups	Age 4X2
BINET IQ[5]	ANOVA[6]	ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA[7]		ANOVA
BINET MA	ANOVA	ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA		ANOVA
BINET SLOPE		ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA		ANOVA
PPVT IQ[5]	ANOVA	ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA		ANOVA
PPVT SLOPE		ANOVA	ANOVA	ANOVA	ANOVA			ANOVA
ACH TEST IV		ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA	ANOCOVA	ANOVA
SCHOOL ACH IV		ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA	ANOCOVA	ANOVA
SCHOOL BEH IV		ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA	ANOCOVA	ANOVA
TASC IV		ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA	ANOCOVA	ANOVA
GASC IV		ANOVA	ANOVA	ANOVA	ANOVA	ANOCOVA	ANOCOVA	ANOVA

[1] Analyses of variance and covariance tables are listed in the appendix, Tables IV - XXVII.

[2] Categories from Table 1.

[3] Attendance - Classification with four levels: 1) Non-experimental, 2) Low attending experimental, 3) Medium attending experimental, 4) High attending experimental.

[4] With trend analysis over four testings (trials).

[5] Fourth testing IQ except for Primary X Trials, where IQs from I, II, III and IV testings are used.

[6] ANOVA - Analysis of Variance.

[7] ANOCOVA - Analysis of Covariance.

It was not thought necessary to perform analyses of variance and covariance on every possible combination of dependent and independent variable because of the obvious consistency of the data. Nevertheless, in order to go one step further, not so much for the sake of hypothesis testing as to descriptively reveal the total picture of the experimental, non-experimental comparison, a multiple regression analysis was performed using the experimental, non-experimental dichotomy as the dependent variable and performing step-wise multiple regression analysis using all of the fourth testing variables as well as BINET slope, PPVT slope, sex, and chronological age. In this kind of analysis, positive findings are relatively meaningless because of the contrived and post hoc nature of variable selection. However, the presence of clearly negative results is quite meaningful and can offer fairly convincing evidence for a total lack of between-group differences. This is because the mathematical manipulation gives weightings which maximize the correlation between the dependent variable (experimental x non-experimental) and a linear combination of the independent variables. The fact that no possible combination of the independent variables can significantly discriminate the groups is fairly convincing evidence for not rejecting the null hypothesis. This approximates a multivariate test of significance.

The use of multiple regression analysis is a special case of multiple discriminant function analysis in which the obtained beta weights give a unique and maximal solution to the problem of group differences. The results of this multiple regression analysis were negative in that the multiple R was .55 with the F for the residual being 1.04. The F never approached significance throughout the step-wise analysis. Thus, the multivariate analysis of group difference is completely consistent with the univariate **analyses reported** in Table 4.

Of signal importance with regard to the hypotheses of this study was the results of the BINET and the PPVT over the four testings. These were analyzed using analyses of variance as indicated in Table 4 under "primary X trials" (Appendix, Tables IV, V and VI). The means and standard deviations associated with those analyses of variance are presented in Tables 5 and 6. As was mentioned above, group differences did not reach statistical significance, which was also true of the "group X trials" interaction. There is a predominant linear trend for these tests over the four testing periods and, for both the BINET and the PPVT, the main effect of "trials" is significant. This significant yearly increase in the total mean score of the sample on both tests suggests a confounding of regression effect from the subnormal mean of the original sample of scores, standardization flaws, and real changes that have taken place in these lower-class children over the three-year period in which they were tested. This is particularly important because of the large number of studies of lower-class children that do not use any carefully chosen control groups and, therefore, are subject to the usual threats to validity associated with changes in the test performance of children that are spuriously attributed to particular main effects (interventions).

Prediction of Fourth Testing Variables

A straightforward way of viewing longitudinal effects is to deal with the pragmatic problem of prediction. We are here interested in both the theoretical problem of behavioral stability as well as the practical problem of finding out

Table 5

Stanford-Binet L-M (1960) IQ, Means and Standard Deviations[1]: Four Testings and Slope[2]

Group	TESTING				Slope: Four Testings
	First 1962	Second 1963	Third 1964	Fourth 1965	
Experimental N=38	92.6 12.91	99.1 13.41	97.7 10.85	97.7 13.97	15.1 29.55
Non-Experimental N=21	89.2 13.75	91.9 10.00	95.4 13.30	96.3 13.71	24.4 46.44
Total N=59	91.4 13.31	96.5 12.79	97.0 11.82	97.2 13.89	18.4 36.74

[1] Means are entered in upper left and standard deviations in lower right of each cell.

[2] Slope = $-3X_1 - X_2 + X_3 + 3X_4$ (Linear). Second and third order slopes were not calculated.

Table 6

Peabody Picture Vocabulary Test IQ, Means and Standard Deviations[1]: Four Testings and Slope[2]

Group	TESTING				Slope: Four Testings
	First 1962	Second 1963	Third 1964	Fourth 1965	
Experimental N=38	78.0 12.11	86.2 14.60	90.7 15.93	92.4 14.37	47.5 47.38
Non-Experimental N=21	74.9 11.15	82.3 20.05	89.5 19.14	90.0 12.59	50.6 36.81
Total N=59	76.9 11.88	84.8 16.81	90.3 17.13	91.5 13.81	48.6 43.94

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[1] Means are entered in upper left and standard deviations in lower right of each cell.

[2] Slope = $-3X_1 - X_2 + X_3 + 3X_4$ (Linear). Second and third order slopes were not calculated.

the least amount of testing that can give us the most amount of information. We are already well apprised of the fact, as has been discussed above, that information about a particular intervention did not contribute to our understanding of later test and school behavior. Now we are faced with the more general question of what kinds of antecedent information are most useful for predicting school and test behavior.

In order to deal directly with this problem, a series of multiple regression analyses were performed using fourth testing cognitive measurements as dependent variables and, respectively, first, second and third testing measures as independent variables. The results of 15 separate multiple regression analyses are reported grossly in Table 7 and, in detail, in the Appendix, Tables XXVIII, XXIX and XXX.

Table 7

Multiple Correlations of First, Second and Third

Testing Variables with Fourth Testing Variables[1] (N=59)

Fourth Testing Dependent Variable	Multiple R's[2]		
	First Testing	Second Testing	Third Testing
BINET IV	75**	80**	84**
PPVT IV	58**	63***	66***
SCHOOL ACH IV	41	53*	54*
SCHOOL BEH IV	53*	55**	57**
ACH TEST	41	47	45

1) Tables of beta weights can be found in Appendix, Tables XXVIII, XXIX and XXX.

2) Significance of "F" test for residuals. Levels of significance: * - .05
** - .01
*** - .001

It is rather striking how trivial the changes in the multiple R's were from year to year. The second and third testings added little to the efficiency of the first testing variables to predict SCHOOL ACH IV and ACT TEST IV. The relatively high and significant multiple R's for the BINET and the PPVT were principally a function of test-retest correlations of the respective tests (see Table 9).

The predictions of SCHOOL ACH IV and ACH TEST IV were generally insignificant, with multiple correlations between .40 and .50. Although the test-retest stabilities of the BINET and the PPVT lent themselves to relatively good long-term predictions of the same tests when they were administered at later times, the variables of SCHOOL ACH, SCHOOL BEH, and ACH TEST as measured towards the end of the first year of public school, were relatively independent of the prediction variables. Thus, when the prediction was not directly made with any earlier testing of a criterion variable, it generally was weak.

Table 8

Correlations of BINET I, II, III and IV with

Fourth Testing Achievement Variables (N=59)

	BINET I	BINET II	BINET III	BINET IV
SCHOOL ACH IV	32	38	44	41
SCHOOL BEH IV	36	29	37	36
ACH TEST IV	31	34	35	32

The correlations of SCHOOL ACH, SCHOOL BEH, and ACH TEST with each of the four testings of the BINET are listed in Table 8. It can readily be seen that the fourth testing inter-correlations of the BINET with the three measures of achievement are no greater than the correlations of achievement with either

BINET I, BINET II, or BINET III. This is further documentation of the inference that the power of the BINET to predict achievement is invariant over time for this sample. Another way of looking at this is in terms of the BINET test-retest correlations. To the extent that they are very high, subjects will be ordered in the same way from one testing to another. This test stability will put a ceiling on the ability of such a test to adjust its predictiveness over time. If achievement is effected by more transient behavioral affects, it will be relatively independent of BINET IQ.

Stability of Aptitude Variables

Correlations of the BINET, PPVT, ITPA and VSMS over the four testings are listed in Table 9.

Table 9

Correlations of Aptitude Variables: BINET, PPVT, ITPA, VSMS (N=59)

	BINET				PPVT				ITPA	
	I	II	III	IV	I	II	III	IV	II	III
	1	2	3	4	5	6	7	8	9	10
1 BINET I		74	68	65	71	66	59	53		
2 BINET II			76	71	71	69	61	49	80*	
3 BINET III				81	63	60	63	55		77*
4 BINET IV					58	64	49	51		
5 PPVT I						64	59	46		
6 PPVT II							63	52		
7 PPVT III								58		
8 PPVT IV										
9 ITPA II										83
10 ITPA III										
11 VSMQ I	42	34	18	13	29	29	37	22		

*ITPA Raw Score and Stanford Binet M.A. with Chronological Age partialed out.

The correlations between the BINET and the PPVT are progressively lower as the children grow older, starting at .71 in the first testing and descending to .51 in the fourth testing, probably an indication of the increasing effective specificity of the PPVT relative to the BINET. It is to be noted that the correlation of BINET I with PPVT II is slightly greater than the first versus second test-retest correlations of the PPVT. These correlations, along with the descriptive information that was discussed above, indicate that the PPVT is quite a different kind of a test for two and three-year-old children than it is for five and six-year-old children. It would appear that, at the earlier age, the PPVT is a test of general intelligence, in spite of the fact that it is much more specific than a global test such as the BINET.

Also of note in Table 9 are the extremely high correlations between BINET MA and ITPA raw score, with chronological age partialed out. Thus, the ITPA has limitations similar to the PPVT in that, for young children, it tends to be a global test of general intelligence and it does not appear to have any specific variance which adds information to that which we already have from the BINET. We found this to be equally true for the ITPA during the second and third testing. Because of practical limitations and the apparent diminishing returns which the test offered, it was not administered during the fourth testing.

BINET correlations over the four testings range from .65 to .81 as compared to .46 to .64 for the PPVT. Thus, the more specific and shorter test is predictably less stable than the longer, global test. Furthermore, the correlations of the BINET with the PPVT (.49 - .71), are comparable to the PPVT test-retests correlations. This suggests the problem of the confounding of reliability with stability. If we wish to study change, maximally reliable measures will interfere because the process of developing precision will necessarily eliminate items that are sensitive to unstable behaviors. Conversely, measures which are more sensitive to change will not pass customary tests of reliability. As valuable as the BINET has been, we would look towards the continued development of meaningful specific tests, such as the PPVT. Such tests will give appearances of being less reliable than more global tests, but they will contribute more to the meaningful assessment of change.

Achievement Variables

The correlations of second, third and fourth testing achievement variables presented in Table 10, reflect a considerable amount of test-retest stability. This drops off for the fourth testing, but ACH TEST IV utilizes tests that are qualitatively different from the reading readiness tests used in the earlier testings. Of note is the near zero correlation of LEE CLARK III with SCHOOL ACH IV and with SCHOOL BEH IV and the modest correlation (.51) between SCHOOL ACH IV and ACH TEST IV. Test performance appears to be fairly independent of teacher rated school achievement and behavior. This is striking in light of the wide variability within the sample (Table 3) and the rather careful directions that were given teachers to specifically rate children on academic standing among peers.

The correlations of BINET IV with achievement variables were small and fairly constant (.31 - .43) from test to test, test to rating, and testing to testing.

The correlations of the achievement variables with each other and with BINET describe an empirical ceiling on our ability to predict school achievement.

Table 10

Correlations of Achievement Variables: Tested and Teacher Ratings (N=59)

	LEE CLARK RR III	METRO RR III	SCHOOL ACH* III	ACH TEST IV	SCHOOL ACH IV	SCHOOL BEH IV	BINET IV	PPVT IV
LEE CLARK RR II*	71	78	56				31	17
LEE CLARK RR III		80	59	34	13	01	43	23
ACH TEST IV					51	24	32	08
SCHOOL ACH IV						62	39	36
SCHOOL BEH IV							34	42

* Experimental only, N=38.

Note: Correlation greater than .25 significant at .05 level.

In order to evaluate the effects of an intervention that takes place over a considerable period of time it is necessary to have knowledge about systematic change of, in this case, achievement variables. But this is just what we have been unable to establish--systematic or predictable patterns of developing achievement. Until we can more adequately account for the covariance that connects preschool with school, we will be hard put to convincingly discuss the efficiency of preschool programs. More concretely, if a design calls for a comparison of two reading methods, a necessary condition is that reasonable predictions of reading success can be made for either method or for both taken together. This problem has been discussed in another context by Kiesler (1966) who calls it the "uniformity assumption myth." The working assumption that all subjects are equally capable of learning how to read will inevitably lead to the failure of studies to show differences, even if they exist. Unfortunately, because of the variety of measurement problems connected with the educational study of preschool children, and the consequent inadequate variable relationships, we were forced to adapt an assumption of relative uniformity.

The comparison of cross-sectional and longitudinal data, that have been presented in this section and in Tables 7, 8, 9 and 10, implicitly raises several rather crucial questions regarding the collection of meaningful data. We are, obviously, not concerned here with test-retest correlations of a particular test but rather with our ability to predict and, therefore, understand the ingredients of successful school behavior. The fact that we can produce relatively high cross-sectional correlations is of rather trivial consequence if we cannot demonstrate that the relationships shown have longitudinal significance. There is much in our data to indicate that the longitudinal relationships that do exist are not particularly important for understanding school success.

We have noted a mild but consistent tendency for children with higher IQs to do better in school, but what is noteworthy is that the efficiency of the intelligence tests to predict achievement does not increase over the four testing periods, in spite of the fact that the test-retest correlations rise from .65 (first versus fourth) to .81 (third versus fourth). Thus, increasing the precision of the BINET does not, correspondingly, increase its predictive efficiency. One might argue that while the BINET variance is due to both more and less stable components, the ability of the BINET to predict achievement rests only on the more stable common components of predictor and criterion measures.

The interesting question that this raises is whether those children who are successful (in a relatively unpredictable fashion) can be expected to have success-correlated IQ gains in their academic years ahead. Looking back at the data, the correlations between slopes and current achievement are practically zero (Table 11) which indicates that presently successful children in school are not typified by any particular slope pattern in their prior behavior.

It might be, of course, that in the years to come successful achieving children in school will turn out to be children with higher slopes, i.e., those children's IQs will tend to go up and low achieving children's IQs will tend to go down. What is fairly certain, however, is that the consistently positive but relatively small correlations of intelligence and achievement which have been found over a period of three years and over several different measures, leave considerable room for variation in performance and are not consistent with the relationships between intelligence and achievement of older elementary age children as they go through the grades.

Table 11

Correlations of Fourth Testing Variables ($N=59^1$)

		2	3	4	5	6	7	8	9	10	11	12
1	BINET IV	51	13	23	39	34	32			07	11	42
2	PPVT IV		-05	14	35	42	06			08	09	48
3	TACS IV			47	09	07	02	36	01			10
4	GACS IV				11	14	01	25	08			31
5	SCHOOL ACH IV					51	51	12	10	-02	-17	31
6	SCHOOL BEH IV						24	01	18	04	07	33
7	ACH TEST IV							00	-13	03	-12	23
8	BINET SLOPE								07	-23	-12	06
9	PPVT SLOPE									-13	-06	13
10	SIB ACH IV										62	52
11	SIB BEH IV											26
12	FAMILY IV											
13	FAM ACH IV											52

1) N=44 for SIB ACH and SIB BEH correlations, and N=49 for FAM ACH correlations.

Note: Correlation greater than .25 significant at .05 level.

It is axiomatic that correlations are as much a function of group variation as they are of variable relationship. It could be argued that the sample under study is fairly homogeneous and the relatively low correlations between aptitudinal measures and achievement measures are more an indication of group homogeneity than they are of the inability of the BINET to predict school achievement or to be correlated with it at a coincidental testing period. Furthermore, the three-year period that the four testings cover can be said to have been fairly stable in that the lives of the children in the study did not change appreciably over that period of time. A family evaluation made at the time of the first testing correlated with a family evaluation made at the time of the fourth testing .62 (Table 12) which, considering the type of measurement and the problems inherent in evaluating homes, is quite extraordinary. The implication of this is quite inconsistent with the argument that the children in the study sample are relatively homogeneous and that this homogeneity is reflected by the inability of the BINET to predict academic achievement. The home conditions over the three-year period were characterized by measurable differences which persisted over time. Furthermore, the relatively consistent and high (and expected) test-retest correlation of the BINET and the PPVT over the three-year period indicates a considerable amount of subject variance within the sample and over time, and thus suggests that the relatively low correlations of the BINET with achievement, and the fact that the predictive correlations are no higher than the cross-sectional ones, can be said to be a clear indication that we have to look beyond the BINET in the assessment of the academic potential of children similar to those in our study sample.

Furthermore, the test-retest correlations of the BINET are practically identical with those found by Sontag, Baker & Nelson (1958) in spite of the fact that they report mean IQs of approximately one hundred twenty for their 3, 4, 5, and 6 year-old children. They reported standard deviations quite similar to the ones that we found and the size of their group (50) is practically the same as ours.

Contemporaneous Correlates of School Success

Partly by design and partly by circumstances, we sought and were able to obtain several measurements during the fourth testing which added considerably to the battery of tests that were used in one or more testings. The additional data collected during the fourth testing have already been discussed in Chapter IV, and listed in Table 2. Correlations of all fourth testing variables can be found in Table 11. We have already discussed the relationships between the various cognitive variables in our consideration of longitudinal effects. It can be seen in Table 11 that several non-cognitive measures, the Children's Anxiety Scales, were not correlated with any of the cognitive variables and were, therefore, of little value in analyzing the school success of this particular group of children.

During the fourth testing, the project staff interviewed the teacher of each study child and the teachers of each of the sibs of every study child so that a generalized measure of family school achievement could be obtained. In accumulating these data we considered the average achievement of the siblings both with (FAM ACH) and without (SIB ACH) the study child. The correlations of SIB ACH and FAM ACH with other fourth testing variables are presented in Table 11. SIB ACH has a near zero correlation with the SCHOOL ACH of the study child as well as with the ACH TEST score of the study child. On the other hand, both SIB ACH and FAM ACH correlate .52 with FAMILY IV. Thus, although the achievement of the study child is not correlated with the achievement of the rest of the family and it bears only a limited relationship to FAMILY IV (.31), there is a relatively strong relationship between the total evaluation of the family and the average achievement level of the children in the family. This important relationship is broken down in much more detail in Table 12.

Table 12

Correlations of Variables Concerned with Home and Family. N=59

	1	2	3	4	5	6	7	8	9	10	11	12
1 Warner Scale I		49	50	41	32	18	40	38	35	42	41	42
2 Family I (Home)			63									62
3 Family I (Education)												39
4 Adequacy IV		62	47		69	67	78	92	85	89	84	95
5 Parents Recep. of Child IV		42	32			70	43	70	71	58	83	78
6 Ind. Behavior of Child IV		40	17				46	75	87	65	74	80
7 Marital Relationship IV		68	36					73	63	79	60	79
8 Mother's Behavior IV		61	41						86	87	85	95
9 Mother-Child Rel. IV		51	24							81	83	92
10 Family Solidarity IV		55	31								76	91
11 Mother Attitude to Child IV		55	46									91
12 Family IV		62	39									
13 School Ach IV	12	14	15	28	40	21	21	31	26	21	34	31
14 School Beh IV	16	15	05	27	39	33	21	27	34	27	29	29
15 Binet IQ IV	24	37	17	39	43	46	22	45	44	27	36	42
16 PPVT IV	21	29	12	39	43	54	38	45	52	33	43	48
17 Ach Test IV	04	13	18	22	32	16	15	18	16	12	28	23
18 Fam Ach IV		33	32	47	50	49	33	43	49	41	57	52

Note: Correlations greater than .25 significant at .05 level.

The eight family scales of the four testings are highly intercorrelated with correlations ranging from .43 to .92 and with a median correlation of .76 (Table 12). The median correlation of the scales with the total family evaluations score is .91 and they range from .78 to .95. It can be seen that the family evaluation scales can be explained, for the most part, by one dimension which is strongly weighted by those scales having to do with the mother's behavior, and our perception of her relationship to her family.

It was rather disappointing to find that both BINET slope and PPVT slope were independent of all measures of family functioning and of the Children's Anxiety Scales. We were not able to predict the direction and magnitude of IQ changes in spite of our knowledge, over time, of family functioning and our measures of non-cognitive functioning. Changes in a child's BINET IQ or PPVT IQ over a three-year period must be attributed, therefore, either to errors of measurement or to extraneous systematic variance which we were unable to control for or to measure directly. At this time we would have to conclude that, in spite of the obvious limitations of the measurements used, measurement errors would appear to be the principal cause of individual variation and that any single IQ measure is as useful as the linear combination of the four. This goes back to the question of using instruments that are excessively reliable, to the exclusion of those that would be sensitive to other than developmental changes.

These fourth testing correlations are quite inconclusive and are presented in this section for the purpose of speculating about the directions for future interventions and data gathering procedures. But in the absence of carefully collected data from other sources these correlations and resulting speculations represent, for the time being, an approximation of the relative contributions of cognitive, non-cognitive and environmental effects to the successful school performance of lower-class children. If the results of this study persist as the study children go through the elementary school, then we will be forced to speculate that we need different kinds of norms for children from different kinds of environments. At any rate, these issued will be further discussed in Chapter VIII.

Chapter VIII

Summary and Discussion: School Behavior of Disadvantaged Children

Introduction

Had American psychologists taken Binet's theories and speculations concerning the educability of intelligence as seriously as they did his test, it is probable that we would have, today, a much clearer idea of the relationship between mental retardation and cultural deprivation and, in general, the correlates of social class and intelligence. Further, it is entirely possible that we would have developed environments powerful enough to intrude upon the lives of selected groups of intellectually disadvantaged children in ways that would reduce their massive inabilities to comprehend and profit from the school setting. Binet was not the first scientist to propose the notion that intelligence is educable, i.e., it is modified by training and practice. As we discussed in an earlier chapter, there is a curious history of efforts to educate intelligence, of which the first pedagogical record available is the nineteenth century work of Jean-Marc Gaspard Itard. The story of Victor, The Wild Boy of Aveyron, is familiar (Itard, 1962; Silberstein & Irwin, 1962). However, Itard's principles of treatment are central to this discussion and, therefore, should be mentioned. Itard was convinced that man, left unattended, is inferior to the domestic animal and, without human contact, cannot develop as well as an animal. Further, man's imitative instinct is the force which educates his senses and is most powerful during early childhood and decreases with age.

In 1961, we received a grant from the Cooperative Research Branch, United States Office of Education, to study the "Effects of Non-Automated Responsive Environments on the Intellectual and Social Competence of Educable Mentally Retarded Children." We proposed to locate a group of preschool children drawn from lower-class strata of our society and to provide them with a variety of experiences calculated to engender and reinforce attitudes, motivations, and cognitive skills considered prerequisites for normal intellectual and academic growth. More specifically, we were concerned with some of the ways in which intervention into the preschool lives of lower-class children might reduce the likelihood that such children would develop intellectual and academic deficits--i.e., mental retardation--so frequently found in youngsters from such background

Several characteristics of this research may differentiate it from prior and current related investigations:

1. Our subject population was carefully and systematically randomized into experimental and control groups. However, in view of the probability that control children received special treatments and educational opportunities and discounting the notion that they were truly a "null" group, they were later designated as a "non-experimental" group. On the other hand, a fair number of experimental children were not afforded the fullest opportunities to participate in the experimental program. Therefore, although execution clearly was imperfect, an experimental design was maintained.

2. In the formal evaluations of subjects, "blinds" were rigorously developed and upheld. Our tests of "blinds," utilizing examination of psychologists who administered the various protocols employed, disclosed naiveté on their part as to which children were or were not experimental.

3. Subject attrition was insignificant. After initiation of the intervention programs, the project staff was able to control attrition on subject population. Of 74 children who were originally selected and took part in the project, including both the pilot sample of 14 and the main group, all completed their participation in the formal intervention years. Of the 60

children from the main sample, only one child was lost during the one year follow-up of children in public school.

4. A very high degree of parent cooperation was maintained throughout the course of the study. For example, two days prior to the completion of the project, 41 parents attended a social gathering with the total project staff. Included were non-experimental as well as experimental families. This assemblage contrasted rather dramatically with a recent attempt by several parents to organize a parent-teacher association in a local public school. At such a meeting, held in that school (with an enrollment of 800 children) and attended by some of our own staff, only a handful of parents were present and not one teacher or school administrator attended.

5. From a theoretical standpoint, it was our hypothesis that in studying cultural deprivation we were also studying factors that give rise to mental retardation. Several recent studies have made distinctions between true mental retardation and pseudo-retardation, i.e., psychometric subnormality is indicative of mental retardation only if it is diagnosed as being irremedial. It was our contention that all children whose performances are mentally retarded are, in fact, mentally retarded. As Binet pointed out long ago, mental retardation is a state of current subnormal intellectual functioning. A child who behaves intellectually subnormal--even though he may have an intact central nervous system and may be categorized as "culturally deprived" or "cultural-familial mentally retarded"--is as mentally retarded as another child with demonstrable brain damage and who is functioning at approximately the same intellectual level.

6. In testing the research hypothesis that intelligence is educable and, at least in some instances, low intelligence is a manifestation of a deprived

cultural experience, we were presented with certain design problems. The study of deprivation and its relationship to social and school performance must, by its very nature, be either partially or wholly retrospective. We cannot randomly assign children to experimental and control groups and then systematically deprive the experimental children of certain experiences in order to observe the effects of that deprivation. Our legal and moral codes demand that we utilize existing cultural-educational situations and design studies that give us indirect insights into the effect of deprivation. Stated another way, the proper study of deprivation would examine the null hypotheses that certain kinds of social and intellectual deprivation will not cause differences between groups of children who are exposed and those who are not exposed. Instead, we are forced to study a less satisfactory null hypothesis: that children from a deprived living situation will not benefit from a stimulating school curriculum. This hypothesis is less satisfactory because it puts the burden of proof on the curriculum that is used rather than on the deprivation which is the object of the study. The significance of this latter approach is that deprivation cannot be systematically controlled and therefore, cannot be considered to be a true experimental main effect. The main effect was the presence or absence of a preschool program upon children described as deprived.

In summary, then, our study encountered methodological problems and utilized certain procedures that may warrant the attention of other research workers engaged in similar endeavors.

Summary and Implications of Data

It is appropriate, at this time, to discuss and speculate about the data presented and analyzed in previous chapters. This study did not demonstrate major differences on objective criteria between experimental and non-experimental groups. This conclusion leads one to consider other studies that have recently reported significant increments in measured intelligence and other indices of intellectual competency, working with children of approximately the same chronological ages and socio-economic backgrounds and utilizing "stimulating interventions" to obtain objectives similar to ours. Most recent studies have been hazy in their reporting of subject attrition, adequate control groups, randomization of subjects into various treatment groups, and the utilization of "blinds" in the assessment of children. Absence of any of the above factors may present an illusion of change. For example, positive changes in experimental groups (which our study is able to demonstrate), without direct comparison with randomized controls, can provide the researcher with hazardous results. Further, there is good reason to believe that bias, resulting from prejudicial knowledge about the group membership of a subject being tested, will affect scores. This study has again made us witness to the extraordinary difficulty and complexity in conducting "clean" field research--notwithstanding attention to the aforementioned factors involving controls and "blinds." The following may be illustrative of the almost necessarily muddied nature of non-laboratory research.

In the spring of 1964, our project staff learned, for the first time, that we may have unwittingly created an unwanted "quasi-experimental" group affecting a significantly large number of so-called non-experimental children. Quite ironically, our success in gaining the confidence of parents of experimental children and persuading them to remain in the project (instead of the alternative decision, the public school kindergarten) caused the development of a unique situation in the local public school. On several visits to this school, we learned that, as a direct result of our project--the removal of large numbers of children who otherwise would have attended that school's kindergartens--these classes were able to maintain enrollments of less than 10 children in each. Added to this complicating and unexpected situation was our observation that the kindergarten program at that school--at least partially resulting from these very desirable enrollments--appeared to be a highly effective and enriching one.

Although all of the above presents tantalizing and, to be honest, painful evidence of the vicissitudes and complexities of behavioral research, and although we can do no more than analyze grossly and speculate about the conditions this irony created, it may be important to ask--somewhat embarrassingly at this point--which children were the experimental and which were the non-experimental, if this distinction can still be made. The obvious implication of this question leads directly to our decision to analyze changes in our study population, irrespective of placement in one or another of the original treatment groups.

During the course of our research we realized there was a partial loss of control of our experimental-null group design. Subsequent data analyses revealed that variables, other than the presence or absence of direct interventions, were correlated with intellectual and academic changes among study children--in both positive and negative directions. The above finding, perhaps our most significant contribution to the study of the nature and correlates of intelligence, leads to the salient conclusion of this study.

As discussed in a previous chapter, it was found that deprived children from the most unstimulated and disordered homes began our project with lower abilities, as measured in our formal evaluations, than did deprived children from more stimulating homes. Further, the children from the homes with greater family integrity maintained their superiority--without regard to whether or not they received special educational interventions. The conclusion is obvious although not consonant with current federal and community policy and action. The mere attendance at a preschool program for disadvantaged children does not appear to be, in and of itself, sufficient to prevent the massive learning and other disabilities associated with the effects of cultural deprivation.

The Economic Opportunity Act of 1964, other federal and state legislation, and the swelling civil rights movement have resulted in heretofore unheard of attention to and financial support of programs for the disadvantaged. Although all of these programs must understand the important influence families have on children, there has been no substantial involvement with the home. Instead they have dealt directly with children in traditional educational or neutral settings. Obtaining from what we have observed to be a deliberate strategy is the recent national emphasis on Head Start programs and the current attempts to bus deprived children into what are probably more favorable suburban school settings. Both of these aforementioned programs partially result from strong convictions that intensive and superior educational interventions can circumvent or ameliorate cognitive and motivational disorders certain children develop as a consequence of living in deprived communities.

Inferences from our data revealed that disadvantaged children are influenced more by the home setting than by the external manipulation of their school environment. In light of what we believe to have been the face validity of an enriched preschool program, the inability of this program to produce measurable differences between experimental and non-experimental children causes us to suggest that it is not enough to provide preschool disadvantaged children with an enriched educational opportunity. Families need a great deal of help toward becoming stronger and better integrated units to provide more powerful stimulants and models for intellectual attainment. To the degree this growth is necessary for families, it is equally important for the community to change, including its complex array of systems and sub-systems, styles and character.

Unfortunately, in spite of the very best of intentions, a fostering of certain misconceptions concerning disadvantaged children and their families may have encouraged a relative isolation of these children from their families during the course of special school-centered programs. Notwithstanding these misconceptions, our evidence showed that the home's influence was associated strongly with school success. The probability that the school can exert a positive effect on the child would appear to be meager unless involvement with the home ameliorates those social and cultural conditions that served as antecedents to the cognitive and motivational deficits these children presented to the school.

What are these misconceptions? First, an argument against involvement with parents is based on the notion that parents of disadvantaged children are not particularly interested in the education of their children, and would not be very cooperative in adopting more positive roles in preparing their children for school. Our experiences, and those of others (Mackler & Giddings, 1965; Weiner & Murray, 1963), argue against this assumption. Rather, these parents are often anxious to cooperate with schools in presenting a stronger family influence on the child. However, oftentimes the complexities of school organization and the requirements for stimulating home environments are foreign to the background of the parents and cause them to behave in ways which the schools call inappropriate. All too often these parents are victims of the same conditions we are now trying to prevent with their children. Whatever inappropriate influence they exert, insofar as school is concerned, may be more often due to their lack of knowledge of the requirements expected of them as parents and the generalized effects of an overwhelmingly depressive environment that they too are inhabitants of, than to their unwillingness to cooperate or their disinterest or--as has been startlingly suggested on occasion--their lack of affection and feelings of responsibility for their children. We contend that lower-class parents oftentimes have high levels of aspirations for their children's education. Many differences between these and middle-class parents lie not in the desire but, rather, in the attitude certain parents and their children have that educational goals can be attained (Weiner & Murray, 1963).

Another popular misconception is related to the general problem of "labeling." One might conclude that, in view of their common label, "culturally deprived," these people are alike. There is a great heterogeneity both within and between families who meet criteria as culturally deprived. The degree to which we can positively intrude into the lives of these families depends upon numerous complex and interrelated factors which facilitate beneficial interaction with some families and cause difficulty with others. A blanket endorsement or condemnation of early and continued involvement with families who are culturally deprived demonstrates both an unwarranted prejudice and a grossly naivé and doctrinaire approach to an extremely variable and relatively unstudied group of human beings.

The third and fourth misconceptions are related, albeit antithetical. On the one hand, some have the idea that the intellectual deficiencies of large numbers of culturally deprived children are due to a multiple genetic inheritance that causes these children to be born with inferior central nervous systems, resulting in school failure and mental retardation. On the other hand, there are those who contend that numerous children have normal intellectual development during the early years but that subsequent subnormality stems from requirements imposed by the schools and the alienation between the home and the school culture. Both of these assertions mitigate against an early involvement with families. If the behavior is due to an inherited subnormality it would appear to be of little value to intervene with the family for one would be intervening with comparably subnormal parents and, secondly, it hardly appears likely that one could prevent or reverse inherited subnormality. On the other hand, if school failures related to cultural deprivation are due to those conditions that obtain after the child enters school, an appealing strategy would provide for circumvention of the home, concentration on special programs to more adequately prepare children to ingest the school culture and to help schools become more tolerant of these children. We have provided some evidence to discourage the multiple genetic causation theory of school failure and to support the contention that impoverished early environments cause severe learning deficiencies.

Discussion

As mentioned earlier, we reject the research hypothesis of this study: that a two-year intervention with preschool lower-class children will enhance their demonstrated educability. This hypothesis was tested with a variety of measurements over a three-year period and included the testing of cognitive, non-cognitive and environmental factors. The analyses of the data led to the unequivocal inference that the groups were no more different at the conclusion of the study than they were at the beginning. This can be viewed either in terms of the failure of the intervention as an effective force in the lives of the experimental children, the failure of the measuring instruments to register differential changes in functioning over a three-year period, or of our inability to maintain a true experimental design (discussed in the previous section). This section will explore the implications of these alternative explanations of our results with the explicit goal of setting the stage for future research in this general area.

The measurements used were comprehensive both with respect to substance and technique. To summarize what was discussed in detail in Chapter IV, data were obtained in many contrasting ways: tests directly administered to children; rating scales where the information was supplied by parent, teacher or psychologist; measurements which covered specified testing periods and measurements which represented ratings of a child or a series of direct measurements over an extended period of time; tests of specific abilities and tests of global abilities; measurements which were concerned directly with school behavior and those that were concerned with behavior in a testing situation; single measurements which were obtained in any one of the four testing periods and repeated measurements which were obtained two, three, or four times on each child; and measurements from the domains which we have classified as cognitive, non-cognitive and environmental. Thus, our testing program did not depend upon either one kind of test or one kind of administration of a test.

Given the particular sample of children and the curricula, both of which were described in previous chapters, and given the variety of measurements over an extended period of time, the rejection of the research hypothesis represents an internally valid inference. We do not have any evidence that all curricula would fail to produce changes in all kinds of children from lower-class homes or other kinds of environments (problems of external validity) but we do feel confident that the study here reported as a valid test of our specific hypothesis, throws considerable light on generalized problems of external validity.

Again, although we are forced to reject the research hypothesis which applies specifically to the sample that we studied and to the curricula that we used, we need not necessarily reject the generalized hypothesis about the educability of lower-class children, or the even more generalized hypothesis about the educability of any children. This discussion will be concerned only with the former--how our results apply to the educability of lower-class children, particularly with reference to the possibilities for intervention during the preschool and early school years.

The problem of how intelligence manifests itself and the extent to which it is affected by behavior, on one hand, and the extent to which it unfolds, on the other, is a spectre that is continually in front of us. However, there is an implicit contradiction between the possibilities that children can change in response to specific teaching techniques and the probabilities that these changes will be reflected by global indices of functioning, such as the BINET. For example, aptitudinal tests are constructed in line with an operational principle of stability. Items that show relative variability over time are rejected in favor of items that are more stable. The result is an apparent stability of global measures which is only an indication of test-specific stability and is not necessarily an indication that children do not change. Pointedly, this means that when a child takes a test, and is scored according to where he stands with his age peers, the chances are not very great that he will change with respect to these same age peers over a period of years. Obviously, he is changing and during the preschool years he is changing very rapidly. The apparent lack of change is not a reflection of his own growth but of his position in a frequency distribution.

The importance of global measures of scholastic aptitude, or of "intelligence" as this is commonly called, has to do with efficiency to predict future academic performance. More specific measures of ability do not have a comparable level of efficiency, even when the prediction is to be made within the specific area that is being tested. For example, the BINET is reputed to be a better predictor of future mathematical or verbal ability than, respectively, ability tests in the areas of mathematics or verbal performance. It does not necessarily follow that the BINET is measuring a more innate quality, although many writers have thought about the problem in these terms. However, the BINET has been developed to be stable in the way indicated above and this stability is reflected in high predictive efficiency with respect to other kinds of test or non-test behavior which relate to intellectual functioning. Therefore, it follows that, in order to have an instrument which is highly predictable of some future behavior, it is necessary that the instrument concern itself with behavior that is extremely stable over time. Just the opposite requirement is called for in tests which are designed to be sensitive to changes that take place in individuals. These latter tests will tend to be relatively unstable over time and will result in minimal ability to predict future performance. It should be pointed out here that tests which are measuring non-stable factors are not necessarily unreliable although this certainly presents problems for the test constructor as he must utilize psychometric and statistical techniques of measuring reliability which are more or less independent of the time factor.

The problem that is being alluded to here concerns the obvious dilemma of any curriculum to provide specific developmental and, if necessary, remedial activities for children, which attend very carefully to fragments of the reading process, the quantitative process, or whatever intellectual discipline is being taught, as opposed to those procedures which are more global in their design and execution and which treat larger units of behavior at each stage of the teaching-learning process. This does not mean that the more global approach to teaching does not involve specific attention to the pieces of learning behavior but that these are not the main focus. Furthermore, although it is important that some pieces be attended to, it does not make too much difference which pieces they are.

These remarks about intelligence and about its measurement, on one hand, and about curriculum and its application, on the other, are a general introduction to the main topics of this discussion which concern the question of whether the results that were obtained in this study and which, at this point, are assumed to be internally valid, have external validity--i.e., whether they have general application in the more or less prescribed area of the preschool education of lower-class children. In order to find some resolution to this rather crucial problem we see four rather distinct areas for discussion. The remainder of this section will deal with each. The first three are concerned with problems of curriculum, timing, and sampling of children for the study. The fourth concerns problems in measuring changes in children over a period of time with instruments that have been designed to measure relatively stable factors.

1. Curricula intervention. Many questions can be raised about the optimal strategies in a preschool program for lower-class children. The evolving curriculum of this study was clearly teacher-dominated, although there was extensive collaboration among the investigators and all of the teaching staff. The variation of the Responsive Environment provided the study with a methodology which, by design, was not teacher-dominated and which depended upon rather detailed instructions which specifically aimed at minimizing teacher variability. Of course, it is possible that in spite of the care that was taken to specify the procedures of the Responsive Environment, the teacher may still have dominated the learning situation.

It might be that an optimal strategy would call for comprehensive programming throughout the schooling of lower-class preschool children so that the curriculum would be dominated by various theoretical considerations which are independent of variations either between different teachers or within any one teacher over a period of time. Certainly, the overriding philosophy of the principal investigators of this project was that the curriculum should not be pre-ordained but that it should be developed by teachers working with children in response to the individual children and to the inter-personal relationships between the teacher and the children and within the groups of children. A great deal of attention and energy was given to observing teachers and children and to holding seminars with the teachers regarding the kinds of programs that they were developing and the kinds of methods they were using.

Alternatively, other investigators such as Bereiter and his colleagues (1965), have discussed the use of a tightly prescribed curriculum which is dominated by considerations other than the personal curricular development of particular teachers. For example, the prescription can go in the direction of specific operant techniques applied to particular learning sequences or it can be concerned with a therapeutic climate in the classroom similar to that described by Rexford (1949).

In light of the above discussion we must certainly raise the question of whether our interventions of a preschool program and a Responsive Environment provided a sufficient test of the hypothesis of educability. Perhaps much greater attention should be paid to more extensive and systematic variations of interventions which either minimize or use teacher variation and which utilize measurements that are directly related to curricular procedures. These problems of measurement and the extent of teacher or methodology domination have always caused us much concern. In a very real sense, any teacher-dominated curriculum will not readily lend itself to any direct kind of measurement. The consequent dependence upon global measurement suggests a possible inherent dilemma in designing studies that attempt a comparison between variations along the continuum of teacher domination.

A second criticism that can be made of the curriculum concerns the focus of the entire program on the preschool environment rather than specifically attending to the education and treatment of the entire community or of the families within the community. Although we have been continually aware of the limitations of an intervention which restricts itself, for the most part, to dealing with children in a school situation, it was not within the plans of this study to treat families or to deal extensively with evaluations of siblings and other children in the community. Therefore, the curriculum was concerned with the Responsive Environment and the preschool program with peripheral attention to families during occasional meetings and home visits by teachers.

It might very well be that the failure of this program, as described above, was due to the failure of the intervention to affect total family behavior. Since we found a relatively high correlation between a measure of family adequacy and the average school performance of all siblings, the inference follows that school failure is family linked and must, therefore, be family treated.

2. Timing and duration. It is possible that the failure of the intervention to produce demonstrable results in the experimental children was due to either the timing of the program in the lives of the children or to the total duration of the program for individual children. There is also the possibility that different aged children might respond to different kinds of interventions in diverse ways. In any event, timing has an effect when considered in conjunction with curricular strategies, sampling variation, and measurement problems. For example, children of different ages might benefit differentially from several methodologies.

3. Sampling of children. We have some reason to believe that children selected did not provide a sample of lower-class children which would be most advantageous for testing the research hypothesis of educability. The families of the study children were fairly stable and, consequently, we were able to maintain 59 of 60 subjects during the three-year period, which is certainly rather extraordinary for a study of lower-class children.

Some investigators have reported that mobility is so high in some lower-class schools that there is sometimes as much as 100 per cent turnover in classrooms in a single school year. The families of the study children tended to stay in the same geographic area and, for the most part, in the same houses. They,therefore,had opportunity to receive different kinds of continuing services from private and public agencies, and the schools were able to maintain contact with families and children. Thus, although the neighborhood is clearly lower lower-class and the families that live in this neighborhood have been classified as extremely impoverished, depending largely on welfare assistance, the general nature of the community may have been such as to support the intellectual growth of the children. If this were so, and if this sample of children were not educationally deprived, than one might not expect an experimental intervention, such as the one provided, to produce any demonstrable results.

Along the same line of reasoning, and again in spite of the fact that the neighborhood under consideration was rated as a lower lower-class neighborhood, we see an important discrepancy with respect to sampling strategy. The area from which we drew our families is not surrounded by other lower lower-class neighborhoods. Rather, it is a pocket within a city which is surrounded by a variety of neighborhoods, universities, and business areas. None of our families were geographically distant from either social agencies, hospitals, stores, or universities. There have been city planning projects, university programs, religious group involvement, social work, two well-established neighborhood house programs, and many other kinds of service activities over the past 15 years.

It is difficult to speculate on the effects of such sampling variation but we do feel that we, unwittingly, did not obtain as educationally disadvantaged a sample of homes and children as we wished in order to maximize the possibilities for a valid test of educability. The more deprived a child is the more likely it is that he will respond to an intervention. This reasoning follows directly from the rationale for this study which maintains that negative deviations from normal functioning are likely to be associated with family and educational deprivations. We are not going against the generality of the thesis of educability but rather that we think, for the time being, that this thesis can best be demonstrated with the most severely deprived children.

4. Measurement problems. Measurement problems will always plague investigations such as this one. The day-to-day intervention which involves a variety of teachers and children is a different kind of substance than the very specific and highly reliable tests that are often used to measure the effects of interventions. When these measurements are concerned with abilities that are closely tied to developmental factors,then the apparent relationship between interventions and criteria is spuriously high. It is more parsimonious to conclude that the increased level of ability of individual children over periods of time is due more to their growth over the period of that time than to any intervention that has taken place, whether it be school or a particular kind of remedial or therapeutic sequence.

This is partially a question of precision as it applies interactively to different kinds of measurements. A child's growth in mental age refers to his increasing ability to respond to items on a test as he grows older. The concept of "chronological" age, as it is used in psychometrics, refers to the average performance of children at any particular chronological age. The development of most tests of aptitude and ability hinges upon the changes that take place over periods of time either directly or as can be inferred from the performance of a cross-sectional sample of children. Our study has focused upon the question of whether the slope of developmental growth can be affected by an intervention. We have raised the question of whether the developmental acceleration of particular children can be systematically varied by providing those children with specific interventions. Now it is altogether possible that developmental levels can be changed but that the measurement of this change is hidden in the relatively small amount of variance that is left over after chronological age is literally partialed out. As a matter of fact, the residual variance that remains is not grossly different from that which must be attributed to error. Therefore, relatively little is left for the measurement of changes in children, particularly when these children have more or less normal developmental slopes.

Conclusion

During the past few years, programs for the disadvantaged have mushroomed. Within recent months, we have been privileged to examine--in some instances, at first hand--the curricula and activities of approximately fifty preschool projects for disadvantaged children (See Bibliography). In New York City, Chicago, Oakland, California, Norfolk, Baltimore, Ypsilanti, and in Boston--as well as in many other large metropolises and small hamlets--state colleges, large universities, and state education departments banded together with local communities and voluntary organizations to design and develop programs for these children. Although each program was to some extent unique, both in philosophical orientation and curriculum design, there was a common focus and expectation. That focus was on direct involvement with children in order to prevent an accumulation of learning and motivational disorders. Other common elements observed in most programs had to do with the conviction that early involvement with children was more desirable than delaying intervention until formal school entrance. Most programs did not seek deep and continuous activity with parents and other family members; more recently, one or two newer programs have sought such involvement. A few programs have--a priori--designed very specific curricula to prevent or remediate specific deficits; most programs developed more global curricula, this development taking place concurrently with involvement with children. However, all programs--more or less--appeared experimental and open-minded. That is, there are few hard and fast rules, either in the development of a theoretical position or in the execution of educational interventions with the deprived.

Insofar as formal experimental programs investigating the characteristics of deprived children, their genesis, and preventative and therapeutic programs designed to deal with mental retardation, learning disorders, and character defects are concerned,research activities are not nearly as extensive as those in service programs, nor have these efforts been as satisfactory. The major factors attendant to the quality of significant research in this field are two-fold. First, there are much more modest and restricted funds available for basic and applied research than for program implementation. Secondly, when research is funded, it appears as if each project is bedeviled with a massive array of strategic and tactical problems. In general, these have to do with researcher bias, the development of adequate research controls, notorious subject attrition, and the unavailability of suitable measurement instruments and tests. Therefore, until research workers can develop more tightly controlled studies and replications of studies--an extraordinarily difficult assignment for research with any group of human beings and, especially, with so complex and heterogeneous a group as the "culturally disadvantaged"--educational practitioners must, by necessity, continue to utilize interventional strategies that have "face validities" and must continue striving for a program climate dominated by open-mindedness and control over strong personal pre-judgements and pre-limitations of what might be accomplished under the best of conditions and what might reasonably be expected under modestly effective ones.

Insofar as our present study is concerned, at this time we have neither significant nor convincing data to substantiate our central hypothesis that intelligence is educable. However, this study revealed to us that we still have a great deal to discover concerning the nature-nurture interaction, about the most efficient and sufficient period to begin interventions, and about the varieties of possible intervention models that may have the greatest desired effects. What we do have encourages us to continue the quest for processes and methodologies to educate intelligence and, for certain children, to prevent mental retardation.

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APPENDIX

APPENDIX

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*Key for Abbreviations can be found in Table 2 in Chapter 4.

Groups are abbreviated as follows:

E - Experimental (N=38)

N-E - Non-Experimental (N=21)

RE - Responsive Environment (N=18)

N-RE - Non-Responsive Environment (N=20)

n.a. - Not applicable

Levels of significance:

* - .05
** - .01
*** - .001

TABLE I

Descriptive Statistics* for Second Testing (May, 1963)
Principal Sample (N=59)

1	2	3	2+3	4	2+3+4
Measure	Responsive Environment (Experimental) N=18	Non-Responsive Environment (Experimental) N=20	Experimental N=38	Non-Experimental N=21	Total N=59
Chronological Age (years)	4.0 .57	4.0 .60	4.0 .58	3.8 .54	4.0 .57
BINET IQ	96.5 13.04	101.5 13.30	99.0 13.41	91.9 10.00	96.5 12.79
PEABODY IQ	88.3 17.00	84.9 12.45	86.6 14.94	81.0 18.43	84.6 16.46
ITPA (raw score)	66.3 31.81	70.7 26.40	68.5 29.24	60.5 21.92	65.7 27.18
LEE CLARK RR (raw score)	20.6 11.81	20.5 10.94	20.6 11.37	n.a.	n.a.
TEST TAKING	3.1 .51	3.1 .67	3.1 .60	2.9 .68	3.0 .63
SOCIOGRAM	2.8 .93	3.0 1.40	2.9 1.19	n.a.	n.a.
ABSENCES	32.2 22.27	25.7 17.17	28.9 20.08	n.a.	n.a.

*Means are entered in upper left and standard deviations in the lower right of each cell.

TABLE II

Descriptive Statistics* for Third Testing (July, 1964)
Principal Sample (N=59)

1	2	3	2+3	4	2+3+4
Measure	Responsive Environment (Experimental) N=18	Non-Responsive Environment (Experimental) N=20	Experimental N=38	Non-Experimental N=21	Total N=59
Chronological Age (years)	5.0 .56	5.0 .60	5.0 .58	5.0 .54	5.0 .57
BINET (IQ)	96.4 9.72	98.9 11.70	92.7 10.85	95.4 13.30	96.9 11.82
PEABODY (IQ)	89.4 13.52	91.9 17.84	90.7 15.93	89.5 19.14	90.3 17.13
ITPA (raw score)	100.1 23.10	109.1 26.29	104.7 25.20	104.2 26.37	104.5 25.61
LEE CLARK RR (raw score)	34.6 11.00	37.3 10.56	36.0 10.86	35.4 11.93	35.8 11.25
METRO RR (raw score)	45.2 13.28	42.4 14.45	43.7 13.96	42.9 15.33	43.5 14.46
TEST TAKING	3.0 .86	3.3 .78	3.2 .83	3.2 .96	3.2 .88
SOCIOGRAM	2.6 1.31	2.8 1.09	2.7 1.20	n.a.	2.7 1.20
ABSENCES	33.4 37.89	17.9 23.75	25.4 32.39	n.a.	29.0 35.34
SCHOOL ACH (rating)	3.8 .35	4.6 .30	4.2 .32	n.a.	4.2 .32

*Means are entered in upper left and standard deviations in the lower right of each cell.

TABLE III

Descriptive Statistics* for Fourth Testing (May, 1965)
Principal Sample (N=59)

1	2	3	2+3	4	2+3+4
Measure	Responsive Environment (Experimental) N=18	Non-Responsive Environment (Experimental) N=20	Experimental N=38	Non-Experimental N=21	Total N=59
BINET (IQ)	96.7 13.65	99.2 14.16	98.0 13.97	97.0 13.71	97.7 13.89
BINET (MA)	5.8 .90	6.0 .97	5.9 .94	5.9 .92	5.9 .93
PPVT (IQ)	89.9 12.49	94.7 15.53	92.4 14.37	90.0 12.59	91.5 13.81
TASC	10.1 6.73	12.0 6.45	11.1 6.65	13.9 5.53	12.1 6.42
GASC	15.3 9.26	13.7 9.62	14.4 9.49	16.4 8.43	15.1 9.17
LS	4.9 1.97	3.9 3.16	4.4 2.7	5.2 3.17	4.7 2.91
DS	17.8 7.97	18.0 11.24	17.9 9.83	20.0 7.19	18.6 9.03
PPVT SLOPE	40.3 45.87	54.0 47.79	47.5 47.38	50.6 36.81	48.6 43.94
SCHOOL ACH (rating)	29.1 7.50	30.7 8.12	29.9 7.87	25.7 11.47	28.4 9.53
SCHOOL BEH	30.2 7.87	30.5 8.24	30.3 8.07	30.0 8.49	30.2 8.22
Chronological Age (years)	6.0 .56	6.0 .60	6.0 .58	6.0 .52	6.0 .56
ACH TEST	3.0 1.11	2.8 1.12	2.9 1.12	2.6 1.18	2.8 1.15
FAMILY	42.0 18.66	47.1 14.42	44.7 16.76	39.5 15.79	42.8 16.60
BINET SLOPE	11.4 31.06	18.5 27.69	15.1 29.55	24.4 46.44	18.4 36.74

*Means are entered in upper left and standard deviations in the lower right of each cell.

TABLE IV

Analysis of Variance and Trend Analysis
Stanford-Binet, L-M, 1960, (IQ)
Four Testings

Source of Variance	DF	Sum Sq.	Mean Sq.	F
Testings	3.	1387.88	462.63	9.61***
Linear	1.	966.63	966.63	13.58***
Quad	1.	361.29	361.29	8.95***
Res	1.	59.96	59.96	1.82
Groups: E x N-E	1.	716.22	716.22	1.33
Subjects	57.	30652.01	537.75	11.17***
Group x Testings	3.	264.51	88.17	1.83
Linear	1.	76.51	76.51	1.07
Quad	1.	85.30	85.30	2.11
Res	1.	102.70	102.70	3.12
Error	171.	8230.11	48.13	
Linear	57.	4054.76	71.14	
Quad	57.	2300.91	40.37	
Res	57.	1874.44	32.88	

TABLE V

Analysis of Variance and Trend Analysis
Stanford-Binet, L-M, 1960, (MA)
Four Testings

Source of Variance	DF	Sum Sq.	Mean Sq.	F
Testings	3.	25642.62	8547.54	734.51***
Linear	1.	25533.15	25533.15	1237.99***
Quad	1.	104.44	104.44	13.66***
Res	1.	5.02	5.02	.75
Groups: E x N-E	1.	97.36	97.36	.45
Subjects	57.	12313.57	216.03	18.56***
Group x Testings	3.	42.69	14.23	1.22
Linear	1.	12.99	12.99	.63
Quad	1.	15.67	15.67	2.05
Res	1.	14.02	14.02	2.11
Error	171.	1989.94	11.64	
Linear	57.	1175.60	20.62	
Quad	57.	435.63	7.64	
Res	57.	378.70	6.64	

TABLE VI

Analysis of Variance and Trend Analysis
Peabody Picture Vocabulary Test (IQ)
Four Testings

Source of Variance	DF	Sum Sq.	Mean Sq.	F
Testings	3.	7755.98	2585.33	25.03**
Linear	1.	6965.84	6965.84	69.79**
Quad	1.	765.36	765.36	7.01*
Res	1.	24.78	24.78	.24
Groups: E x N-E	1.	421.35	421.35	.68
Subjects	57.	34919.82	612.63	5.93***
Group x Testings	3.	37.16	12.39	.11
Linear	1.	6.69	6.69	.06
Quad	1.	1.75	1.75	.01
Res	1.	28.72	28.72	.28
Error	171.	17658.12	103.26	
Linear	57.	5688.82	99.80	
Quad	57.	6215.64	109.05	
Res	57.	5753.65	100.94	

TABLE VII

Analyses of Variance of BINET (IQ) IV with Adjustment for Unequal Cells

Source of Var.	Sum Sq.	Adj. Sum Sq.	DF	Mean Sq.	F
Group: E x N-E	26.643	29.433	1.	29.433	.153
Sex	374.102	376.892	1.	376.892	1.955
Interaction	115.436	112.645	1.	112.645	.584
Residual	10600.497	10600.497	55.	172.736	
Total	11116.677		58.		
Sex	374.102	380.292	1.	380.292	1.919
Group: RE x N-RE x N-E	117.034	123.224	2.	61.612	.311
Interaction	120.025	113.835	2.	56.918	.287
Residual	10505.517	10505.517	53.	198.217	
Total	11116.677		58.		
Grade	24.499	31.072	1.	31.072	.157
Group: RE x N-RE x N-E	117.033	123.606	2.	61.803	.312
Interaction	481.794	475.222	2.	237.611	1.200
Residual	10493.351	10493.351	53.	197.988	
Total	11116.677		58.		
Group: E x N-E	26.643	25.195	1.	25.195	.122
Age	15.318	13.870	2.	6.935	.034
Interaction	160.867	162.315	2.	81.157	.394
Residual	10913.850	10913.850	53.	205.922	
Total	11116.677		58.		

TABLE VIII

Analyses of Variance of BINET(MA) IV with Adjustment for Unequal Cells

Source of Variance	Sum Sq.	Adj. Sum Sq.	DF	Mean Sq.	F
Group: E x N-E	1.886	2.326	1.	2.326	.027
Sex	135.623	136.063	1.	136.063	1.564
Interaction	194.848	194.408	1.	194.408	2.234
Residual	4785.744	4785.744	55.	87.014	
Total	5118.102		58.		
Sex	135.623	136.869	1.	136.869	1.523
Group: RE x N-RE x N-E	16.038	17.284	2.	8.642	.096
Interaction	202.663	201.418	2.	100.709	1.120
Residual	4763.777	4763.777	53.	89.883	
Total	5118.102		58.		
Grade	1384.134	1405.512	1.	1405.512	20.676***
Group: RE x N-RE x N-E	16.038	37.416	2.	18.708	.275
Interaction	115.184	93.806	2.	46.903	.690
Residual	3602.745	3602.745	53.	67.976	
Total	5118.102		58.		
Group: E x N-E	1.886	.520	1.	.520	.007
Age	1310.664	1309.298	2.	654.649	9.336***
Interaction	89.286	90.652	2.	45.326	.646
Residual	3716.265	3716.265	53.	70.118	
Total	5118.10		58.		

TABLE IX

Analyses of Variance of PPVT (IQ) IV with Adjustment for Unequal Cells

Source of Variance	Sum Sq.	Adj. Sum Sq.	DF	Mean Sq.	F
Group: E x N-E	107.908	108.958	1.	108.958	.608
Sex	13.003	14.053	1.	14.053	.078
Interaction	223.722	222.672	1.	222.672	1.242
Residual	9860.994	9860.994	55.	179.291	
Total	10205.627		58.		
Sex	13.003	15.164	1.	15.164	.085
Group: RE x N-RE x N-E	370.133	372.293	2.	186.147	1.039
Interaction	331.555	329.394	2.	164.697	.920
Residual	9490.937	9490.937	53.	179.074	
Total	10205.627		58.		
Grade	111.225	88.914	1.	88.914	.536
Group: RE x N-RE x N-E	370.133	347.821	2.	173.911	1.048
Interaction	925.398	947.709	2.	473.855	2.854
Residual	8798.871	8798.871	53.	166.016	
Total	10205.627		58.		
Group: E x N-E	107.908	121.062	1.	121.062	.665
Age	234.683	247.837	2.	123.918	.680
Interaction	207.737	194.582	2.	97.291	.534
Residual	9655.300	9655.300	53.	182.175	
Total	10205.627		58.		

TABLE X

Analysis of Variance of Standardized Rating on Murphy-Durrell Diagnostic Reading Readiness Test and Metropolitan Achievement Test, Primary Level (TEST ACH IV), with Adjustment for Unequal Cells

Source of Variance	Sum Sq.	Adj. Sum Sq.	DF	Mean Sq.	F
Group: E x N-E	1.414	1.388	1.	1.388	1.005
Sex	.671	.645	1.	.645	.467
Interaction	.099	.124	1.	.124	.090
Residual	75.952	75.952	55.	1.381	
Total	78.136		58.		
Sex	.671	.655	1.	.655	.459
Group: RE x N-RE x N-E	1.793	1.777	2.	.888	.623
Interaction	.134	.150	2.	.075	.052
Residual	75.539	75.539	53.	1.425	
Total	78.136		58.		
Grade	.000	.003	1.	.003	.002
Group: RE x N-RE x N-E	1.793	1.795	2.	.898	.651
Interaction	3.286	3.284	2.	1.642	1.191
Residual	73.056	73.056	53.	1.378	
Total	78.136		58.		
Group: E x N-E	1.414	1.635	1.	1.635	1.254
Age	5.198	5.419	2.	2.709	2.079
Interaction	2.437	2.217	2.	1.108	.850
Residual	69.086	69.086	53.	1.304	
Total	78.136		58.		

TABLE XI

Analyses of Variance of SCHOOL ACH IV with Adjustment for Unequal Cells

Source of Variance	Sum Sq.	Adj. Sum Sq.	DF	Mean Sq.	F
Group: E x N-E	239.358	236.585	1.	236.585	2.569
Sex	45.445	42.673	1.	42.673	.463
Interaction	7.187	9.960	1.	9.960	.108
Residual	5064.416	5064.416	55.	92.080	
Total	5356.407		58.		
Sex	45.445	42.113	1.	42.113	.444
Group: RE x N-RE x N-E	261.793	258.461	2.	129.231	1.363
Interaction	25.503	28.836	2.	14.418	.152
Residual	5023.665	5023.665	53.	94.786	
Total	5356.407		58.		
Grade	499.744	455.301	1.	455.301	5.463*
Group: RE x N-RE x N-E	261.793	217.350	2.	108.675	1.304
Interaction	178.116	222.560	2.	111.280	1.335
Residual	4416.753	4416.753	53.	83.335	
Total	5356.407		58.		
Group: E x N-E	239.358	252.625	1.	252.625	2.905
Age	334.636	347.903	2.	173.951	2.001
Interaction	174.018	160.750	2.	80.375	.924
Residual	4608.395	4608.395	53.	86.951	
Total	5356.407		58.		

TABLE XII

Analyses of Variance of SCHOOL BEH IV with Adjustment for Unequal Cells

Source of Variance	Sum Sq.	Adj. Sum Sq.	DF	Mean Sq.	F
Group: E x N-E	2.054	1.417	1.	1.417	.021
Sex	322.951	322.313	1.	322.313	4.839*
Interaction	.508	.129	1.	.129	.002
Residual	3663.062	3663.062	55.	66.601	
Total	3987.559		58.		
Sex	322.951	322.112	1.	322.112	4.663*
Group: RE x N-RE x N-E	2.546	1.707	2.	.854	.012
Interaction	.639	1.477	2.	.739	.011
Residual	3661.424	3661.424	53.	69.083	
Total	3987.559		58.		
Grade	197.733	195.607	1.	195.607	2.819
Group: RE x N-RE x N-E	2.546	.420	2.	.210	.003
Interaction	109.146	111.272	2.	55.636	.802
Residual	3678.134	3678.134	53.	69.399	
Total	3987.559		58.		
Group: E x N-E	2.054	1.825	1.	1.825	.027
Age	385.462	385.233	2.	192.616	2.853
Interaction	21.523	21.752	2.	10.876	.161
Residual	3578.520	3578.520	53.	67.519	
Total	3987.559		58.		

TABLE XIII

Analyses of Variance of TASC IV with Adjustment for Unequal Cells

Source of Variance	Sum Sq.	Adj. Sum Sq.	DF	Mean Sq.	F
Group: E x N-E	103.174	100.232	1.	100.232	2.590
Sex	117.514	114.573	1.	114.573	2.961
Interaction	.025	2.916	1.	2.916	.075
Residual	2128.320	2128.320	55.	38.697	
Total	2348.983		58.		
Sex	117.514	115.537	1.	115.537	2.915
Group: RE x N-RE x N-E	126.924	124.946	2.	62.473	1.576
Interaction	3.649	5.626	2.	2.813	.071
Residual	2100.896	2100.896	53.	39.640	
Total	2348.983		58.		
Grade	.185	.787	1.	.787	.019
Group: RE x N-RE x N-E	126.924	127.525	2.	63.763	1.566
Interaction	64.214	63.612	2.	31.806	.781
Residual	2157.661	2157.661	53.	40.711	
Total	2348.983		58.		
Group: E x N-E	103.174	98.998	1.	98.998	2.388
Age	31.228	27.052	2.	13.526	.326
Interaction	17.388	21.563	2.	10.782	.260
Residual	2197.194	2197.194	53.	41.456	
Total	2348.983		58.		

TABLE XIV

Analyses of Variance of GASC IV with Adjustment for Unequal Cells

Source of Variance	Sum Sq.	Adj. Sum Sq.	DF	Mean Sq.	F
Group: E x N-E	31.925	33.323	1.	33.323	.351
Sex	82.126	83.524	1.	83.524	.879
Interaction	78.117	76.719	1.	76.719	.807
Residual	5226.409	5226.409	55.	95.026	
Total	5418.576		58.		
Sex	82.126	84.825	1.	84.825	.914
Group: RE x N-RE x N-E	89.567	92.266	2.	46.133	.497
Interaction	329.753	327.054	2.	163.527	1.763
Residual	4917.130	4917.130	53.	92.776	
Total	5418.576		58.		
Grade	22.957	17.848	1.	17.848	.188
Group: RE x N-RE x N-E	89.567	84.458	2.	42.229	.446
Interaction	283.068	288.177	2.	144.088	1.520
Residual	5022.985	5022.985	53.	94.773	
Total	5418.576		58.		
Group: E x N-E	31.925	37.229	1.	37.229	.388
Age	165.822	171.127	2.	85.563	.892
Interaction	138.487	133.183	2.	66.591	.694
Residual	5082.342	5082.342	53.	95.893	
Total	5418.576		58.		

TABLE XV

Analysis of Covariance of SCHOOL ACH IV with Adjustment for BINET (IQ) IV

	Source of Variance			
	Total	Within	Between	F
Sum of Products	3160.067	3035.177	124.890	
Sum of Squares (SCHOOL ACH)	5356.406	5094.613	261.793	1.438
Sum of Squares (BINET IQ IV)	11116.677	10999.644	117.033	.297
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.409	.405	.713	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.284	.275	1.067	
Adj. Sum of Squares	4458.114	4257.104	201.010	
Degrees of Freedom	57.000	55.00	2.000	1.322

Adjusted Means:	Group RE	29.421
	N-RE	30.108
	N-E	25.963

TABLE XVI

Analysis of Covariance of SCHOOL BEH IV with Adjustment for BINET (IQ) IV

	Total	Within	Between	F
Sum of Products	2276.152	2262.088	14.063	
Sum of Squares (SCHOOL BEH)	3987.559	3985.013	2.545	.017
Sum of Squares (BINET IQ IV)	11116.677	10999.644	117.033	.297
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.341	.341	.814	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.204	.205	.120	
Adj. Sum of Squares	3521.514	3519.812	1.702	
Degrees of Freedom	57.000	55.000	2.000	.013

Adjusted Means:	Group RE	30.453
	N-RE	30.046
	N-E	30.138

TABLE XVII

Analysis of Covariance of TASC IV with Adjustment for BINET (IQ) IV

	Source of Variance			
	Total	Within	Between	F
Sum of Products	681.237	687.333	6.096	
Sum of Squares (TASC)	2348.983	2222.059	126.923	1.599
Sum of Squares (BINET IQ IV)	11116.677	10999.644	117.033	.297
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.133	.139	.050	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.061	.062	.052	
Adj. Sum of Squares	2307.236	2179.110	128.126	
Degrees of Freedom	57.000	55.000	2.000	1.646

Adjusted Means: Group	RE	10.237
	N-RE	11.627
	N-E	13.818

TABLE XVIII

Analysis of Covariance of GASC IV with Adjustment for BINET (IQ) IV

	Total	Within	Between	F
Sum of Products	1719.186	1820.533	101.346	
Sum of Squares (GASC)	5418.576	5329.009	89.566	.470
Sum of Squares (BINET IQ IV)	11116.677	10999.644	117.033	.297
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.221	.237	.989	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.154	.165	.865	
Adj. Sum of Squares	5152.705	5027.696	125.009	
Degrees of Freedom	57.000	55.000	2.000	.696

Adjusted Means: Group	RE	16.853
	N-RE	13.875
	N-E	17.054

TABLE XIX

Analysis of Covariance of Standardized Rating on Murphy-Durrell Diagnostic Reading Readiness Test and Metropolitan Achievement Test, Primary Level (TEST ACH IV), with Adjustment for BINET (IQ) IV

	Source of Variance			
	Total	Within	Between	F
Sum of Products	277.084	276.800	.284	
Sum of Squares (TEST ACH)	78.135	76.342	1.792	.657
Sum of Squares (BINET IQ IV)	11116.677	10999.644	117.033	.297
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.297	.302	.019	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.024	.025	.002	
Adj. Sum of Squares	71.229	69.377	1.851	
Degrees of Freedom	57.000	55.000	2.000	.747

Adjusted Means Group		
	RE	3.028
	N-RE	2.750
	N-E	2.594

TABLE XX

Analysis of Covariance of BINET (IQ) IV with Adjustment for FAMILY IV

	Source of Variance			
	Total	Within	Between	F
Sum of Products	5163.135	4915.933	247.202	
Sum of Squares (BINET)	11116.677	10999.644	117.033	.297
Sum of Squares (FAMILY)	16263.627	15657.038	606.589	1.084
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.383	.374	.927	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.317	.313	.407	
Adj. Sum of Squares	9477.562	9456.160	21.402	
Degrees of Freedom	57.000	55.000	2.000	.063

Adjusted Means-Group	RE	96.377
	N-RE	97.864
	N-E	97.376

TABLE XXI

Analysis of Covariance of BINET (MA) IV with Adjustment for FAMILY IV

	Total	Within	Between	F
Sum of Products	3203.593	3118.476	85.117	
Sum of Squares (BINET)	5118.101	5102.063	16.038	.088
Sum of Squares (FAMILY)	16263.627	15657.038	606.589	1.084
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.351	.348	.862	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.196	.199	.140	
Adj. Sum of Squares	4487.061	4480.943	6.117	
Degrees of Freedom	57.000	55.000	2.000	.038

Adjusted Means-Group	RE	58.946
	N-RE	59.153
	N-E	59.709

TABLE XXII

Analysis of Covariance of PPVT (IQ) IV with Adjustment for FAMILY IV

	Source of Variance			
	Total	Within	Between	F
Sum of Products	5852.372	5401.033	451.339	
Sum of Squares (PPVT)	10205.627	9835.494	370.133	1.053
Sum of Squares (FAMILY)	16263.627	15657.038	606.589	1.084
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.454	.435	.952	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.359	.344	.744	
Adj. Sum of Squares	8099.685	7972.360	127.324	
Degrees of Freedom	57.000	55.000	2.000	.447

Adjusted Means:	Group RE	89.681
	N-RE	93.183
	N-E	90.479

TABLE XXIII

Analysis of Covariance of TASC IV with Adjustment for FAMILY IV

	Total	Within	Between	F
Sum of Products	633.847	750.119	-116.271	
Sum of Squares (TASC)	2348.983	2222.059	126.923	1.599
Sum of Squares (FAMILY)	16263.627	15657.038	606.589	1.084
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.102	.127	-.419	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.038	.047	-.191	
Adj. Sum of Squares	2324.279	2186.121	138.158	
Degrees of Freedom	57.000	55.000	2.000	1.769

Adjusted Means:	Group RE	10.207
	N-RE	11.546
	N-E	13.921

TABLE XXIV

Analysis of Covariauce of SCHOOL BEH IV with Adjustment for FAMILY IV

	Source of Variance			
	Total	Within	Between	F
Sum of Products	2620.830	2582.623	38.206	
Sum of Squares (SCHOOL BEH)	3987.559	3985.013	2.545	.017
Sum of Squares (FAMILY)	16263.627	15657.038	606.589	1.084
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.325	.326	.972	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.161	.164	.062	
Adj. Sum of Squares	3565.221	3559.010	6.210	
Degrees of Freedom	57.000	55.000	2.000	.048

Adjusted Means: Group RE 30.362
N-RE 29.748
N-E 30.500

TABLE XXV

Analysis of Covariance of Standardized Rating on Murphy-Durrell Diagnostic Reading Readiness Test and Metropolitan Achievement Test, Primary Level (TEST ACH IV), with Adjustment for FAMILY IV

	Total	Within	Between	F
Sum of Products	258.016	245.114	12.902	
Sum of Squares (MURPHY & METRO ACH)	78.135	76.342	1.792	.657
Sum of Squares (FAMILY)	16263.627	15657.038	606.589	1.084
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.228	.224	.391	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.015	.015	.021	
Adj. Sum of Squares	74.042	72.505	1.536	
Degrees of Freedom	57.000	55.000	2.000	.593

Adjusted Means: Group RE 3.013
N-RE 2.733

TABLE XXVI

Analysis of Covariance of GASC IV with Adjustment for FAMILY IV

	Source of Variance			
	Total	Within	Between	F
Sum of Products	2028.237	2254.647	-226.410	
Sum of Squares (GASC)	5418.576	5329.009	89.566	.470
Sum of Squares (FAMILY)	16263.627	15657.038	606.589	1.084
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.216	.246	-.971	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.124	.144	-.373	
Adj. Sum of Squares	5165.634	5004.335	161.299	
Degrees of Freedom	57.000	55.000	2.000	.902

Adjusted Means: Group		
	RE	16.788
	N-RE	13.587
	N-E	17.383

TABLE XXVII

Analysis of Covariance of School Achievement of Study Child Rated by Teacher (SCHOOL ACH IV) with Adjustment for FAMILY IV

	Total	Within	Between	F
Sum of Products	2913.813	2545.842	367.970	
Sum of Squares (SCHOOL ACH)	5356.406	5094.613	261.793	1.438
Sum of Squares (FAMILY)	16263.627	15657.038	606.589	1.084
Degrees of Freedom	58.000	56.000	2.000	
Correlation Coefficient	.312	.285	.923	
Degrees of Freedom for R	57.000	55.000	1.000	
BXY	.179	.162	.606	
Adj. Sum of Squares	4834.364	4680.658	153.706	
Degrees of Freedom	57.000	55.000	2.000	.919

Adjusted Means: Group		
	RE	29.248
	N-RE	29.958
	N-E	26.254

TABLE XXVIII

Multiple Regression of First Testing on Fourth Testing Variables:
Multiple R, F Tests of Residuals and Beta Weights

Dependent Variable	R	$F_{7,51}$	Independent Variables						
			BINET I	PPVT I	VINELAND I	WARNER I	CA	SEX	RORSCHACH I
BINET IV	75	9.12***	54	16	-13	-15	-08	-23	10
PPVT IV	58	3.65**	38	19	-02	-12	-16	-07	01
SCHOOL ACH IV	41	N.S.	24	-02	-00	-03	-28	04	15
SCHOOL BEH IV	53	2.78*	32	-10	00	-03	-34	21	13
ACH TEST IV	41	N.S.	30	11	-18	-01	19	08	00
FAMILY IV	59	3.96**	17	27	-02	-36	00	13	00
BINET SLOPE	58	3.63**	-43	10	-19	-14	-07	-31	09

TABLE XXIX

Multiple Regression of Second Testing on Fourth Testing Variables: Multiple R, F Tests of Residuals and Beta Weights

Dependent Variable	R	$F_{7,51}$	Independent Variables						
			BINET III	PPVT II	LEE CLARK RR II	CA	SEX	ITPA II	RORSCHACH
BINET IV	80	13.10***	51	15	-15	-13	-25	32	02
PPVT IV	63	4.85***	08	21	-14	-43	-06	59	-05
SCHOOL ACH IV	53	2.85*	07	-15	31	-55	-02	31	07
SCHOOL BEH IV	55	3.16**	-02	-08	-10	-63	23	53	10
ACH TEST IV	47	N.S.	15	03	28	10	-01	09	-05
FAMILY IV	50	2.41*	42	01	-05	02	15	-07	09
BINET SLOPE	45	N.S.	19	-07	-11	12	-43	-08	-13

TABLE XXX

Multiple Regression of Third Testing on Fourth Testing Variables: Multiple R, F Tests of Residuals and Beta Weights

Dependent Variable	R	$F_{7,51}$	Independent Variables BINET III	PPVT III	ITPA III	CA	SEX	LEE CLARK RR III	TEST TAK III
BINET IV	84	17.95***	68	-13	27	-15	-22	04	09
PPVT IV	66	5.61***	15	31	33	-32	-01	-01	-06
SCHOOL ACH IV	54	2.94*	20	-21	49	-51	02	09	-10
SCHOOL BEH IV	57	3.48**	16	-16	34	-55	20	15	-02
ACH TEST IV	45	N.S.	27	04	15	16	06	-02	01
FAMILY IV	56	3.29**	41	22	-06	10	25	-01	-10
BINET SLOPE	60	4.00**	45	-46	-11	-02	-41	22	-06

TABLE XXXI

Correlations of Anxiety Scales with other Variables

	TASC	GASC	LS	DS
GASC IV	47			
LS IV	52	81		
DS IV	55	66	64	
BINET I	17	04	00	06
BINET II	07	18	10	18
BINET III	18	35	29	39
BINET IV	13	23	25	29
BINET SLOPE	36	25	33	35
PPVT I	-12	-01	-04	-09
PPVT II	03	17	17	12
PPVT III	02	08	-01	12
PPVT IV	-05	14	17	11
PPVT SLOPE	01	08	08	17
ACH TEST IV	02	01	08	02
SCHOOL ACH IV	09	11	08	15
SCHOOL BEH III	07	14	14	09
FAMILY IV	14	33	21	19
Chronological Age	04	08	08	03

TABLE XXXII

Pilot Sample (N=14): Means for First and Second Testing

First Testing	Experimental with Typewriter	Control
Binet IQ	96.3	93.1
Peabody IQ	75.1	71.9
Vineland SQ	109.9	104.7
Chronological Age	5.03	5.27
Second Testing		
Binet IQ	96.7	91.3
Peabody IQ	82.7	83.3
Illinois (raw score)	100.4	100.1
Binet Change	.4	-2.9
Peabody Change	6.7	11.4